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to Anthropology:
Band Societies

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CONTRIBUTIONS TO ANTHROPOLOGY:

BAND SOCIETIES

*Proceedings of the Conference on Band Organization
Ottawa, August 30 to September 2, 1965*

Edited by David Damas



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Foreword

This issue of Contributions to Anthropology is devoted entirely to the Conference on Band Organization held August 30 – September 2, 1965 at the National Museum of Canada. The third in a series of conferences sponsored by Ethnology Division, it was planned and organized by Dr. David Damas, Arctic Ethnologist.

The first conference, held in 1963, was concerned with the general question of museums and anthropological research. At that time several programs and projects were formulated, some of which have already been realised, the establishment of a fellowship program for research based on ethnological collections in museums (Wenner-Gren) and pilot projects for inventorying ethnological materials in museums (CARM-AAA).

A second conference, on Algonquian linguistics, was convened in Ottawa in 1964. It is believed that this was the first conference in history devoted entirely to one family of American Indian languages. It was attended by twelve linguists, very nearly the total for all of North America, who are actively engaged in research on Algonquian linguistics. The papers read have since been published in a special issue of the museum bulletin (Contributions to Anthropology: Algonquian Linguistics, Bulletin 214, 1967).

The stimulus to research engendered by these conferences has been most gratifying. It is hoped that the publication of the proceedings will further increase the flow of such contributions to anthropology in the future.

A. D. DeBlois
Chief Ethnologist

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INTRODUCTION

The plan to bring together a group of anthropologists to discuss theories of band organization stemmed from a personal interest in several problems.

Examination of data that I had gathered in the Central Eskimo area raised doubt about conceptions of Eskimo social organization which had appeared in the literature. The groupings I had studied appeared to resemble the sort of aggregates which had been described for other hunting societies as 'bands.' This consideration led me to seek a sharper definition of the concept of the 'band.'

Helm's system of band composition analysis, which she had applied to Déné material, promised to be a valuable tool for examining material from band societies in other parts of the world.

Problems raised in Service's *Primitive Social Organization* appeared to be rekindling interest among anthropologists in the theory of band organization, an interest which had been initiated largely by Julian Steward in the 1930's. Correspondence with several colleagues who had worked with other hunting and gathering groups revealed a general interest in reformulating the concepts of Steward and Service. Discussion of these matters with Dr. Tom F. S. McFeat, then Chief Ethnologist at the National Museum, prompted his suggestion that a conference might well be devoted to problems of band organization theory. Planning proceeded during 1965, and the conference convened August 30 to September 2 of that year.

The following anthropologists attended:

Marco Bicchieri, Beloit College; David Damas, National Museum of Canada; R. W. Dunning, University of British Columbia; Fred Eggan, The University of Chicago; Peter M. Gardner, The University of Texas; June Helm, University of Iowa; Eleanor Leacock, Bank Street College of Education; Lorna Marshall, Cambridge, Massachusetts; Catherine McClellan, University of Wisconsin; Tom F. S. McFeat, University of Toronto; Robert A. McKennan, Dartmouth College; Edward S. Rogers, Royal Ontario Museum; Richard Slobodin, McMaster University; B. J. Williams, University of California, Los Angeles.

The papers presented in the volume were read at the conference with the exception of McKennan's, which is included at the request of several of the participants, and two papers by Julian Steward. Dr. Steward was unable to attend because of illness but sent a paper, which was read at the conference and is included here. Dr. Steward also kindly provided a summarizing paper "Postscript to Bands." In a number of cases, papers were expanded, principally to include more documentation, but seldom were positions taken in the original papers modified to any great extent in rewriting. The complete record of the discussions was taped, transcribed, edited, circulated to the participants for comment, and revised on the basis of the comments. In this part of the work I have followed the general procedure and format of Tax's *Appraisal of Anthropology Today*.

The entire panel of participants are to be thanked for their cooperation during the meetings and throughout the preparation of the manuscript, and special thanks are due Professors Eggan, Helm, McFeat, and Slobodin for agreeing to chair the topical sessions.

DAVID DAMAS, *Editor*

PART I:

THE MONTAGNAIS—NASKAPI BAND

BY ELEANOR LEACOCK

RÉSUMÉ

Qu'est-ce qui est intrinsèque à l'intégration sociale au niveau de la bande? Cette question réclame un essai de reconstitution des sociétés vivant en bandes à l'époque où elles n'avaient pas encore connu le contact d'autres civilisations. Si l'on reconsidère les données recueillies au Labrador, on aboutit aux suggestions suivantes. La localisation dans la parenté virile n'est pas une composante essentielle de l'organisation en bandes, ainsi que Service le suggère. L'unité de base socio-économique est le groupe multifamilial et non la cellule familiale. L'exogamie semble clairement démontrée, mais il demeure difficile de préciser quelles sont les unités exogames parmi les groupements saisonniers en expansion ou en régression. L'indéniable et totale interdépendance des habitants des forêts du Nord postule et permet, tout à la fois, la flexibilité des groupements ainsi qu'un individualisme improprement interprété comme un «atomisme». Si l'on en croit les témoignages clairsemés, il semble que les mariages entre cousins marquent plutôt la «naissance» d'une organisation de clan que la survivance d'anciens groupements à structure unilinéale. Les sociétés des forêts du Nord sont, il est vrai, relativement spécialisées, et il est possible que des sociétés plus stables se soient antérieurement organisées dans un environnement moins difficile.

I should like to preface my discussion of the Naskapi band with some general remarks on history, acculturation, and evolution, as they are presently viewed. First, it is now recognized that aboriginal peoples, especially in the New World, were influenced by European commercialism earlier than was previously assumed. Much of what was formerly considered to be aboriginal represents instead an early phase of acculturation, a relatively viable synthesis of old and new that preceded the severe shaking up or outright destruction of Indian societies. So far as I know, it was William Duncan Strong who first clearly formulated this point. From his Columbia University seminar on "Time Perspective and the Plains" came studies of early Indian-white contacts and influences in the Plains and other areas that laid the basis for much of the ethnohistorical research that followed.

Today it is surprising to remember that Speck and Cooper took 1670 (the date when the first Hudson's Bay Post was established at James Bay) to represent the significant starting point for European influence in Labrador. It is well known that fur trading became a lively enterprise soon after Columbus's first voyage to the New World, and may even, in some slight degree, have preceded it. The Micmacs, who sighted Cartier's ships in 1534, waved furs on sticks to entice the men ashore, and throughout the 16th century there was constant competition among French companies for control of the St. Lawrence trade. Approximately forty years before the establishment of the James Bay Post, Charles L'Allemand wrote:

Before the time of the association of those Gentlemen to whom the King gave this trade for a certain time . . . the Savages were visited by many people, to

such an extent that an Old Man told me he had seen as many as twenty ships in the port of Tadoussac. But now since this business has been granted to the association . . . we see here not more than two ships which belong to it, and that only once a year about the beginning of the month of June. These two ships bring all the merchandise which these Gentlemen use in trading with the Savages; that is to say the cloaks, blankets, nightcaps, hats, shirts, sheets, hatchets, iron arrowheads, bodkins, swords, picks to break the ice in Winter, knives, kettles, prunes, raisins, Indian corn, peas, crackers or sea biscuits, and tobacco. . . . In exchange for these they carry back hides of the moose, lynx, fox, otter, black ones being encountered occasionally, martens, badgers, and muskrats; but they deal principally in Beavers, in which they find their greatest profit. I was told that during one year they carried back as many as 22,000. The usual number for one year is 15,000 or 12,000, at one pistole each, which is not doing badly. (Thwaites 1906, IV: 207)

Many such passages found in the *Jesuit Relations* and other accounts indicate how important early European influence was in this area. I do not think it is necessary to review the argument that one result of this influence was the development of the so-called 'family hunting territory,' actually a trap-line, which represented the 'successful' acculturation phase of Indian history.

Now the historical perspective of the New World takes better account of very early European-aboriginal contacts, and, as the archaeological record has unfolded, a fuller sense of pre-contact complexities has developed. Previously there was a vague notion that the aboriginal societies which we painfully attempt to reconstruct had existed for untold generations as more or less stable entities. Scholars now take a second look at the possible pre-European influences of more complex cultures on simpler ones, and there is a greater sensitivity to the ebb and flow of culture histories as changing ecological conditions affected subsistence patterns, population density, and movement.

As for evolution, today there is no longer great interest in arguing 'evolution' versus 'history,' though there are, of course, many different phrasings of the relationship between the two and a consensus does not exist. I do not know how many of us here today would see 'evolution' as referring to basic and general historical processes, which, when expressed in theoretical terms, enable us more easily to interpret sequences of individual and unique events. In any case, central to evolutionary theory, as it is generally understood, is a statement of the relation between economic arrangements and other aspects of society. Up to a point, such a relation is widely accepted as important; that is, there is fair agreement that the economy sets obvious limits on possible social developments. For example, under unusually favourable circumstances, hunting-gathering-fishing societies can reach a greater degree of complexity than simple agricultural societies, but I doubt if one would argue that they could equal the potential opened up by agriculture and achieve urbanization. Or, as another instance, some may still want to call status differences in pre-urban societies 'classes,' but I believe no one now disputes that such 'classes' are different from classes in urban society.

The argument begins when the economy is understood not just to limit, but also to determine various aspects of social organization. I do not want to introduce a spurious argument about a partly semantic matter, because, of course, limitation is necessarily determination of a sort, and determination

can only be determining limits. However, the issue is one of degree, and of how actively and specifically the economy influences other parts of culture. The concept of determination implies a strong active influence, but it definitely need not imply a mechanically narrow influence, and it certainly should not imply a one-way influence. Deterministic theories based purely on productivity fail to give proper consideration to the relationship established among the members of a society by the form of productive work. On the other hand, nonevolutionary ecological arguments, although they seem to take these relationships into account, can also neglect them. Speck argues that the "family hunting territory" resulted from the nature of available game existing in the forested area below the Height of Land in Labrador compared to the more migratory animals that were found to the north. This is an ecological argument in this narrow sense.

The relationship between the economy and the rest of the society is often discussed in terms of 'stages,' or significantly different types of production techniques, economic arrangements, and related social and political forms. In using the concept, it is important to avoid too static an implication; that is, an overemphasis on the relative equilibrium achieved at each 'level of integration,' at the expense of a dialectical orientation towards conflicting tendencies that lead to a major or revolutionary reintegration. An evolutionary approach generally implies the qualitative leap of Marxian-Hegelian dialectics, though this is not often explicitly stated. From an evolutionary point of view, social development does not simply involve a series of accumulative changes. Instead, there is the point at which a real transformation is effected, and something qualitatively different has developed.

The theoretical problem, which we are discussing in this seminar, is to define the nature of band organization. One aspect is the question of what is intrinsic to the hunting-gathering 'stage.' Service's statement concerning the limits ordinarily set by a hunting-gathering technology expresses, I think, virtual consensus about what can readily be observed, although there would doubtless be arguments about one or another specific aspect of his formulation;

. . . there are no special economic groups or special productive units such as guilds or factories, no specialized occupational groups, no economic institutions such as markets, no special consuming groups or classes. The economy, in short, is not separately institutionalized, but remains merely an aspect of kinship organization; in the usual modern sense of the word, there is no formal economy at all.

The same is true of other cultural functions. There is no separate political life and no government or legal system above the modest informal authority of family heads and ephemeral leaders. Likewise, there is no religious organization standing apart from family and band; and the congregation is the camp itself.

The fact that the family and band are simultaneously the sole economic, political and religious organization greatly influences the character of these activities. The economy, polity, and ideology of the culture of bands is unprofessionalized and unformalized; in short, it is familistic only. (Service 1962: 108-9)

Thus, it is clear what band organization is not. It does not include specialization of labour beyond that based on sex, nor include class divisions, a formal priesthood, or hierarchical political organization. In addition, basic sources of livelihood are not privately owned. More problematic is the

question of what band organization is. A well worked out statement is needed to describe how relationships function in the hunting collective in their own right as well as how they contrast with institutions of urban society. Hickerson (MS.) makes such a statement when he points out that distribution of food is a right, not an obligation (our concept), among simple peoples, and is valued as such.

Service stresses virilocal exogamy as basic to band organization; that is, the structuring of society around reciprocally out-marrying groups that have related male hunters as their core. He writes, "The most significant rules among patrilocal bands are *reciprocal band exogamy* and the associated *virilocal marital residence* mode, for it is these that create the patrilocal structure of the band" (Service 1962: 66-7). Later he elaborates,

Both the internal structure of each band and the external associations among certain bands are created by rules of exogamy and virilocal residence. A "natural" biological group is amorphous except for the dominance hierarchy, sexual pairs, if any, and mother-child dyads. However, once reciprocal virilocal marriage exchanges between two such groups have become modal for more than one generation then the relations between the two groups have a character invested with new, cultural, determinants. The simplest is the common moiety arrangement of two, or often several groups arranged as two sides which intermarry (Service 1962: 69)

Service re-evaluates material on those bands Steward considered to be either "composite" — the Algonkians, Athabascans, Andamanese, and Yahgan — or at the "family level of integration" — the Basin Shoshone and Eskimo. He argues that all were originally structured around patrilocal exogamous units. Service writes that in a hunting-gathering society "a woman is a more 'liquid asset' in a band than is a male," and "the fraternity of males is the unit of most solidarity" (Service 1962: 49-50). "Virilocality is expectable," he feels, "because of the importance of the solidarity of the males in hunting, sharing game, and particularly offense-defense. This necessity could be expected to continue from early to late times, until the epoch of modern acculturation" (Service 1962: 67). Thus, "all the people are each a part of their *father's* group, not the mother's; she remains in some sense an outsider, particularly in the early years of the marriage" (Service 1962: 69).

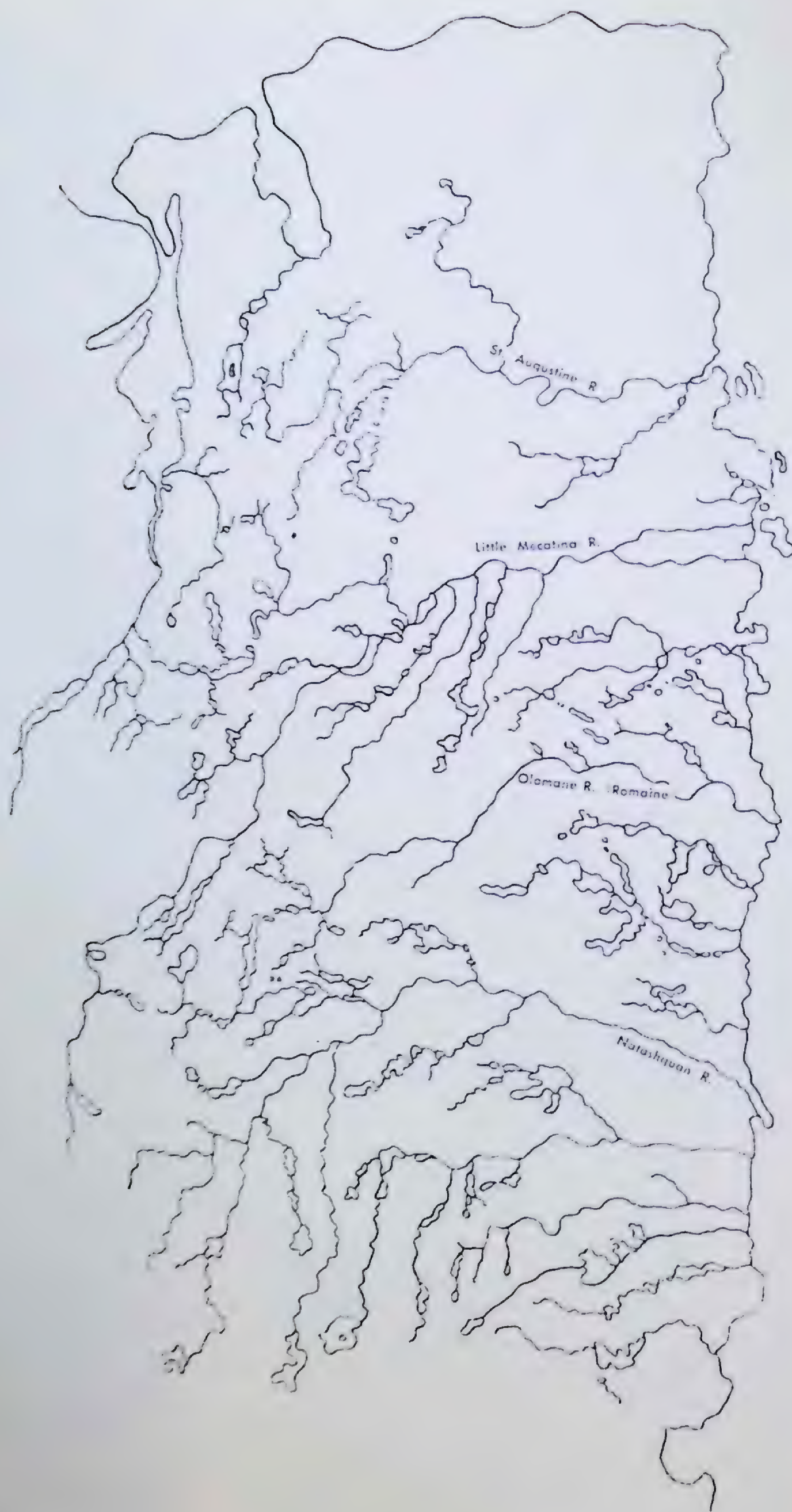
In the Labrador area, however, in no case, early or late, is there evidence for this relatively permanent core of related males who are joined by outside females. There are no suggestions of exclusive virilocality, and until quite recently not even predominant virilocality. None the less, this is an area where hunting was of overwhelming importance, and an area where pressure from the Iroquois to the south and against the Eskimo on the east Labrador coast and the north shore of the lower St. Lawrence River was important from the early post-contact period. Patrilineally related groupings have been strengthened, as I have documented elsewhere (Leacock 1955), while early records, especially the *Jesuit Relations*, supply rich evidence for the prior importance of matrilineal ties. One finds reference to matrilocality as the ideal pattern for the Montagnais (Thwaites 1906, XXX: 169), and reference also to inheritance by the sister's son (Thwaites 1906, VI: 255), as well as many instances where a man either lives with or close to his wife's father

(Thwaites 1906, V: 163; IX: 33; XIV: 143-5; XVI: 49; XXXI: 149; LVI: 151). The closeness of bonds with a son-in-law is revealed in LeJeune's account of a dying man who is being cared for by the Jesuits. The man's wife, "his children, his sons-in-law, his friends, and his fellow-savages, his Manitousiouets, sorcerers, or jugglers" all try to make him come away with them. When he persistently refuses, it is a son-in-law who stays with him until his death (Thwaites 1906, VI: 125). References to the role of women show them to be extremely important in decision making (Thwaites 1906, V: 181; VII: 175; LXVIII: 93). Occasionally they were shamans (Thwaites 1906, VII: 61; XIV: 183) and would sometimes join a hunt (Thwaites 1906, II: 77).

Long after the fur trade and other external pressures toward the patrilineal 'family hunting territory' were introduced, matrilocality remained important in the area. Speck refers a number of times to the prevalence of father-in-law-in-law ties, and shows at least half of Mistassini trapping partnerships to be of this type (Speck 1917: 91, 97-8; 1923: 462; 1927: 392). In Southeastern Labrador, where the 'hunting territory' (or trap-line) system had not yet been established by 1950, one could document a marked shift from prevalent matrilocality band exogamy toward band endogamy and a greater proportion of patrilineally structured hunting partnerships. For instance, there was only one family at Natashquan whose male members had been born there, and although this was seen as a natural course of affairs, it was one that was changing since the younger generation no longer moved away to marry.

Any suggestion that patrilocality increases individual efficiency in a hunting economy, because a man works in an area he has known from childhood, is contradicted by evidence from Labrador. This had been another argument for virilocality. Much travelling about and moving from one band to another has been noted, first by the Jesuits and, in fact, by all recorders of Indian life. There is every indication that this movement is old and is well established. Changes in trading post locations, the intrusion of white trappers and settlers, and the depletion of game have caused shifts in the general distribution of the population, and (in post-contact times) in the yearly cycle. However, the attitudes expressed by my informants about individual movements do not indicate that this is disrupting what were formerly stable groups. Instead, there is a feeling of constantly increasing restrictions, the most recent being against band exogamy, directly discouraged by trading post officials and missionaries, and indirectly influenced by other factors.

Mathieu Medikabo, a man of 60, spoke of the time when the whole southeastern area was "one hunt," with St. Augustine, Musquaro, and Natashquan people crossing over the Height of Land to descend to Northwest River in mid-winter. Now there are too many white people. Mathieu's father had crossed over to hunt in Newfoundland one year; he had found good hunting, but the area had then become subject to "a different government" and it was no longer possible to go there. Mathieu himself had hunted at Musquaro until his first wife died and had lived at Natashquan about twenty years when I worked with him in 1950. His prodigious knowledge of the area is shown by one of the sketch maps he drew for me. (The only map I had at the time left the Labrador interior one large blank.) The



MAP 1 — Southeastern Labrador as Sketched by Mathieu Medikabo.



MAP 2 — Southeastern Labrador.

territory covered is roughly two hundred square miles, yet this map does not exhaust Mathieu's knowledge, for at different times he would select different smaller stream and pond systems to draw in detail, according to the topic of our discussion. Map 2 represents, so far as I was able to determine, the same stream systems Mathieu portrayed in the sketch pictured.¹

Although Mathieu Medikabo's familiarity with his environs encompassed in great detail the territory hunted by about forty men in two bands, it seemed to represent fairly standard knowledge for the older generation with whom I worked. A moment's reflection will make clear how important such knowledge was for survival in the north country. Familiarity with only one area is far too limiting, whereas intimate knowledge about animal habits in relation to types of terrain applies widely and affords a large number of alternative choices for hunting, and greater flexibility of response to changes in the animal population, and to various intra- and inter-group relations. As one informant put it, "everyone Indian, no like'em this one, going to hunt the other one." When the Indians became dependent on the fur trade, they became more tied to their semipermanent lines of steel traps; as hunters they moved about.

In summary, there is no evidence for patrilocality as an organizing principle among the Montagnais-Naskapi. Instead, the evidence favours matrilocality. However, as early as 1632 patrilocality as well as matrilocality tent groups were described by the Jesuits. Was this the result of male-oriented trading ties and the disruption resulting from the Iroquois invasions, or did the exigencies of life in the north woods, with its extremely low population density, always call for the 'expediency' (mentioned by Service as a later consideration) that did not allow a rigid system to be maintained? On the other hand, there is little question about exogamy being the former practice. It persisted until recent times in southeastern Labrador (Leacock 1955: 35-6) and characterized the even more remote Barren Ground and Davis Inlet Bands as they were when Strong worked with them in 1927-28 (Strong, MS.). Information regarding the exogamic unit is, however, lacking for the earlier period. This brings us back to the original question: what was the structure of the Montagnais-Naskapi band?

There is no doubt that the large and loosely organized 'composite' band, comprised of relatively independent nuclear families, first described in detail by Speck, is a recent phenomenon that followed the dependence of the Montagnais on the fur trade (Leacock 1954: 19-23). Earlier groupings in Southwestern Labrador described by the Jesuits in the 17th century, represented seasonal fluctuations in the number of people who could gather at different times of the year. They were of four types, or orders of magnitude: (1) the multi-family group inhabiting one lodge or 'cabin'; (2) several such groups forming a cooperating unit most of the year; (3) the named group usually referred to as the 'band,' which consists of two or more of the previous groupings, inhabiting the same roughly defined territory and uniting, or trying to, for short periods during the summer at the St. Lawrence coast or some large interior lake; and (4) the gathering of people from several such 'bands.' It is the relationship between the second and third types of groups, with respect to exogamous units and post-contact changes, that is

both the most pertinent and the least clear. In any case, for over 350 years the size, composition, and location of the third and fourth categories have increasingly been determined by the location of missions and trading posts, while the first two have been undercut by the individualizing effects of fur trapping and trading.² The relation between the second and third categories is further obscured by the fact that some of the named groups considered to be in category three were perhaps originally in category four. Examples of 17th century references to the four groupings follow.

1. *The multi-family lodge group* (Size, 10–20). One point is clear; the basic socioeconomic unit was not the nuclear family but the multi-family group inhabiting a lodge, although the breaking away of individual families apparently began early in cases where Indians attached themselves to missions or trading posts. The Jesuits referred to “large cabins” that contained “a number of men, women and children” (Thwaites 1906, V: 105). In a few instances the number of people in such lodges was given. LeJeune reported 10, 16, and 19 in the “cabins” he wintered in in 1632–3, with several mature hunters in each (Thwaites 1906, VII: 121). There is a later reference (1673) to 34 persons occupying two cabins (Thwaites 1906, LIX: 31). Elsewhere there is reference to the small size of the average Indian family, with two or three and rarely more than four children, in contrast to the French who may have had eight, ten, or twelve (Thwaites 1906, LII: 49). In isolated areas, the multi-family lodge persisted until late. In 1905, Wallace met a group of Indians on the George River, including 11 adult males, who lived in two “wigwams,” with three fires in the larger one. These Indians, he said, “had not accustomed themselves to the use of flour, sugar, and others of the simplest luxuries of civilization and their food was almost wholly flesh, fish, and berries” (Wallace 1906: 135–6).

2. *The aggregate of several lodge groups, or the winter band* (Size, 35–75). In the fall, several lodge groups left the shore together to go inland. Usually by late December they were forced to separate to hunt over a wider area, remaining close enough to lend mutual aid. LeJeune’s group of 35 split into two on December 24, and when Druilletes went inland in 1647 with a party of 50, they divided around Christmas time (Thwaites 1906: XXXII: 269). In February of 1670, Albenal, travelling with a group of 15 to 20 met with a “cabin of Savages,” and wrote of “two large cabins about 6 leagues from us” (Thwaites 1906, LIII: 73). Contact was also maintained among the larger groups. LeJeune referred to another camp of 45, a day’s travel away (Thwaites 1906, VII: 121), and several times during the winter other Indians came to them for help. In the case of Druilletes, four cabins came from another quarter, where they had been starving (Thwaites 1906, XXXII: 271).

The account of Crespieul’s winter in 1763 in the Lake St. John area illustrates the network of expanding and contracting Indian encampments spread through the north woods. Setting out in late September, Crespieul first stayed with “five cabins” of Papinachois. He then met a large encampment awaiting him at Chicoutimi. He ascended to the interior with “six canoes,” or about twenty-five to thirty Indians who were joined by “four families” of Outabitebecs. “All together we entered the woods, to seek our

livelihood and to meet a great number of Savages who were to come down in the spring." He spent November with "two cabins" of 34 persons, and, before setting off for Hudson Bay, he visited Albenal, who was staying with "four cabins" of Indians. Upon his return, he stopped with "two cabins" of Outabitebs about four miles from his original group. Crespieul returned to prepare for a journey to the Mistassini and Papinachois and revisited Albenal before leaving. In the spring there were warnings of Iroquois raiding in the area, and the Indians he was accompanying gathered and fortified themselves. Later, after making contact with a large camp, he set out for the Coast, encountering "four large cabins" on the way. He arrived back at Chicoutimi at the end of May, where many Indians awaited him (Thwaites 1906, LIX: 27-47). In the fall of 1676, Crespieul left for Lake St. John, and spoke of "five cabins" of Indians from the area who awaited him, as well as "four families" of Algonkins from Three Rivers, and several individuals who came from the woods upon hearing of his arrival. He left on November 30 with "eight families" to "enter the forest" (Thwaites 1906, LX: 247). Assuming that "family" refers to a nuclear family, the camps mentioned vary from the minimal unit of about fifteen people to groups of about seventy-five in addition to the larger numbers that temporarily gathered in warmer seasons.

There are several specific references to the size of the multi-lodge groups, which the Jesuits often referred to as "bands," using the terms "tribe" or "nation" to refer to the larger aggregates. In 1646, it is reported that Indians arrived at Tadoussac "in small bands, one after another." In one case, 200 came from a "single nation," but "these poor people, withdrawing into their forests, usually separate themselves into three bands" (Thwaites 1906, XXIX: 139-41). This would indicate groups of 60 to 70 persons each. In 1642, Vimont reported that 13 canoes, carrying about sixty Atticamegs, arrived in Sillery from their St. Maurice area (Thwaites 1906, XXIV: 67). Nouvel visited the Papinachois, to the east of Tadoussac, some years later. In the fall of 1662, he contacted a group of 68, who had fortified their camp because of reports that Iroquois were in the area (Thwaites 1906, XLVIII: 279). The following spring he met a party of 64 at Lake Manicouagan (Thwaites 1906, XLIX: 49). At Seven Islands, a mission served a "tribe" of 150 people, the Oumamiois, who "go about in two small bands" in the winter (Thwaites 1906, LIX: 57-9).

3. *The named group commonly called the band* (Size, 150 to upwards of 300). Reference has already been made to the Oumamiois group of 150, and a "nation" of 200, the former divided into two "winter bands," the latter into three. On Druilletes's 1647 trip, he left Tadoussac with a group in eight *chaloupes*, or French open boats, and several canoes. The assemblage divided after crossing to the south shore of the St. Lawrence, and Druilletes's party of 50 left two *chaloupes* at the Coast. This would indicate a total aggregate of about two hundred. Druilletes's group returned to the shore on March 3, and by April 14 all had gathered to proceed to Tadoussac and farther up the St. Lawrence River (Thwaites 1906, XXXII: 259-73).

In 1648, the Atticamegs were reported to have arrived in "three bands," the last consisting of 40 canoes, making the total considerably larger than

the previously mentioned groups (Thwaites 1906, XXXII: 283). In 1669, toward the end of May, Albenal arrived at the place where 150 Papinachois were assembled (Thwaites 1906, LII: 85). This may or may not have comprised all Indians called Papinachois, but the following spring contact was made with a group of about the same number (Thwaites 1906, LIII: 85). The next summer, he went to baptize a band on Hudson Bay, and on his return to Lake St. John he met and baptized a party of 150 Mistassini. He later met an assemblage of 200 Indians, whose identity he did not give, and exacted a promise from them to come to Lake St. John the following year. Still later in the summer, the Mistassini band he previously contacted arrived at Lake St. John (Thwaites 1906, LVI: 207-11). The 1681 record refers to the very short periods during which the scattered "tribes" assembled in greater numbers. The Mistassini are mentioned gathering, but only for three weeks, at Lake Kenogami, which is located further down the Saguenay than Lake St. John "after which they separate into small bands, for fear that, by keeping together in too great numbers, they may suffer from hunger" (Thwaites 1906, LXII: 221-3).

Buteaux's trip in the spring of 1652 illustrated that numbers came together to the extent they could. Although in this case the situation was somewhat influenced by the presence of a priest, it was doubtless similar to the older patterns. In March, Buteaux visited about 40 Atticamegs, who split into two parties to make canoes. When finished, his group set out for a place where "all" were gathered, arriving May 18. Several days later, the entire encampment embarked in 35 canoes (about 150 to 160 people), and arrived at "another assembly, about twenty-five leagues hence." Here they feasted on moose, beaver, and bear's fat, and departed for another gathering, three days away, with 60 canoes (or approximately 260 to 280 people). However, "hunger compelled this gathering to disperse" (Thwaites 1906, XXXVII: 19-63).

4. *Gatherings of people from several bands* (Size, up to 1,500). Groups this large gathered at trading posts in the spring and summer. The Atticamegs, who arrived in 1648 in numbers of over 300, met 400 Indians at Three Rivers (Thwaites 1906, XXXII: 283), where a trading post had been established for some thirty years. A post had functioned at Tadoussac, farther down the St. Lawrence River, since 1599. In 1645 there were 350 hunters with their families at Tadoussac, or from 1,200 to 1,500 individuals. (This is not our estimate, but one given in the record and in agreement with the average family size mentioned elsewhere (Thwaites 1906, XXIX: 123).) However this number was reduced by disease, and in the summer of 1650 there is a reference to only 800 Indians having assembled (Thwaites 1906, XXXVI: 223). It is stated that in 1669 there were ordinarily 1,000 to 1,200 Indians at the post, but because of a smallpox epidemic, there were scarcely one hundred left (Thwaites 1906, LII: 85). These gatherings included people other than Montagnais. At Chicoutimi, up the Saguenay from Tadoussac, where a post was established in 1650, Crespien in the late 1670's speaks of Abenaki, Eskimo, and Algonkin families awaiting him, as well as people from a number of neighbouring Montagnais groups (Thwaites 1906, LX: 245; LXI: 85-7). Though the magnitude of these encampments indicates that they

were post-contact, they doubtless had some roots in old patterns of Indian trade and visiting. There is reference, for example, to reciprocal hunting visits between the Montagnais and Abenaki (Thwaites 1906, XII: 187).

In later times, as white settlement increasingly limited the freedom of the Indians to move about their territory, the large summer assemblages in most places became restricted in size, while in the upper St. Lawrence River area they became impossible. By the middle of the 19th century, the two largest gatherings reported by Hind occurred at the Mingan and Seven Islands posts, with 500 and 350 Indians respectively. The westernmost post on the St. Lawrence River, at Tadoussac, served only 100 Indians, and settlements at other posts throughout the Labrador peninsula ranged from 75 to 250 (Hind 1863: 117). Recently, Seven Islands, the railhead for the interior iron mine, has become a rapidly growing centre of both white and Indian populations.

To return to earlier groupings and their relevance to the question of aboriginal band structure: which was the exogamous unit, the smaller fall and spring band or the larger summer band? To what extent was the larger band already affected by the fur trade? Was the 150 to 200 figure, with two or three sub-bands, the earlier norm for one band, and had the Atticamegs, close to an old trading post, already increased in numbers? Buteaux's interior group grew briefly to over three hundred, but could not maintain this size for more than several days, even in the summer. Did this grouping comprise people from two bands, who habitually came together for socializing, trading, and out-marrying? Formerly these people had traded with the Hurons according to Buteaux (Thwaites 1906, XXXVI: 65); doubtless some trade in this area was old. When some Atticamegs invited one of the Jesuits to come to their country, they pointed out that tribes from farther north would be gathering at the same place (Thwaites 1906, XXXVII: 139). Certainly peoples commingled on one another's band 'territory' with great ease. There is a suggestive reference to the mission at Lake St. John where missionaries attended gatherings of the "Porcupine nations," given in the plural (Thwaites 1906, XXXVII: 211). (Their name apparently follows from the prevalence of porcupines in the area. Such animal names (Atticameg, or White-fish) were rapidly replaced by locality names associated with trading posts.)

The occurrence of out-marrying groups at the third level, numbering around 150, agrees rather well with the recently disappearing band exogamy of Eastern Labrador. In that area there were also winter hunting parties akin to the tent-groups, and traces of two band subdivisions at St. Augustine and Northwest River. There was a feeling of closeness among those contiguous bands that most commonly interchanged their members. As previously stated, the pattern was for people from the Natashquan, Musquaro, and St. Augustine bands to cross over the Height of Land and go down to the Northwest River for the summer, and vice versa. Similarly, the northern wing of the Northwest River band apparently formed another intermarrying and interchanging network with the Barren Ground, Davis Inlet, and other northern peoples. The Barren Ground and Davis Inlet people summered together at Indian House Lake. Apparently population was sparser in the northern barrens than in the woodlands to the south, although the extremely small size of the Barren Ground and Davis Inlet

peoples, which, at the time Strong worked with them numbered 56 and 36 respectively, was the result of a severe smallpox and some measles epidemics in 1918 (Strong, MS.).

In the absence of direct evidence, it is impossible to say whether exogamy was a binding or a preferred custom, or whether the minimal exogamous unit was a band subdivision rather than the band. However, whatever the exogamous group might have been, and however strong the exogamous sanction as a principle of organization, exogamy did obtain, as Service suggests. In addition to its persistence in eastern and northern Labrador, there was the occurrence in the northernmost bands of an institution tied in with closely linked exogamous groups; that is cross-cousin marriage, as well as the kin terms appropriate to the practice (Strong 1929). The exogamy was not virilocal, but uxoriocal in emphasis, though how absolute this was remains a further question. In any case, to leave the matter at this, and simply substitute uxoriocal exogamy for virilocal exogamy as the organizing principles of Montagnais-Naskapi band life, is not sufficient. There is the need, mentioned above, for a fuller description of means whereby effective cooperative units are maintained among such peoples. Material on life in the north woods suggests two mechanisms that provide a beginning: complete rather than reciprocal sharing and a great latitude for individual choice. The first makes possible the second, and both seem essential for survival.

The stringency of life in the north woods enforces so immediate an interdependence that sharing is a more total, 'spontaneous,' or 'unstructured' affair than the regulated reciprocity generally obtaining in more settled societies. Starvation is a constantly occurring threat, though usually a fairly localized one, and people do not hesitate to turn to others for help. The Jesuits marvelled at the unstinting, unhesitating way Indians who were themselves in trouble shared with others who were in greater straits. In the hinterland it is still unquestioned that help will always be forthcoming, even from a bitter personal enemy (Lips 1937).

It may be necessary to qualify that in speaking of 'complete' sharing by hunters, it would be absurd to imply that everything, including small catches of fish or rabbits, is always shared. Nor would I deny, in making a differentiation between the sharing of game with everyone present, and sharing whatever there is available in times of trouble, on the one hand, and the reciprocal exchange of simple agriculturalists, on the other, a long-run generalized principle of reciprocity among hunters. But the reciprocity is generalized, and the point seems to lie in the difference between sharing as enabling the dependence of anyone on anyone else and regularized sharing as a means of equalizing the distribution of goods.

I had occasion to question one of my informants, Thomas Gregoire, about a latter day incident where he had given the last of his flour and lard to two men from a neighbouring band. This meant returning to the post sooner than he had planned, thereby reducing his possible catch of furs. I probed to see whether there was some slight annoyance or reluctance involved, or at least some expectation of a return at some later date. This was one of the very rare times Thomas lost patience with me, and he said with deep, if suppressed anger, "Suppose now, not give them flour, lard — just dead inside." More

revealing than the incident itself were the finality of his tone and the inference of my utter inhumanity in raising questions about his action.

With regard to latitude for individual choice, flexibility in the movements of individuals and groups seems indicated from earliest times, with a constant shuffling and reshuffling of personnel to serve several purposes, namely (1) the sensible distribution of the population through the hunting area, (2) the balanced age and sex composition of any group, (3) another aspect of (2), the re-forming of groups in cases of starvation, and (4) the allowance of sufficient choice so that the tent groups, thrown so closely together for such long periods, could be as congenial as possible. (The Jesuits commented on how remarkably well groups lived and worked together, but LeJeune also commented on the deep grudges that occasionally were held.)

The question is whether this movement of families is a 'breaking down' of formerly more stable groups, or simply an 'expedient' response to new conditions, different in its specific content, but not in form, from the 'expediency' always required. In eastern Labrador, moving about is considered desirable, whether to find a spouse, to be with a loved relative or friend, or simply to get to know a new territory. Moves are phrased as matters of individual choice, and friendship is important in determining hunting partners. Patterns of marriage show the formation and re-formation of congenial groups, with weddings of brother and sister pairs, or parent-child pairs (such as a widowed mother and a son marrying into another band at the same time), as common occurrences. As an example of family moving practices, Mathieu Medikabo, the map-maker, came to Natashquan at the same time as his step-sister. She "married Natashquan," a patrilocal move when considered by itself, yet with her came not only her mother, brother, and step-brother, all widowed and with marriage in mind, but also her sister and sister's husband and children!

Whether such flexibility was or was not pre-Columbian has important implications for our problem of determining what is intrinsic to the social organization of hunting and gathering peoples (or, more accurately, hunting and (or) fishing and (or) gathering). More is known about the upper limits of hunting and gathering economies where the predictability, accessibility, and sufficiency of food resources are roughly equivalent to that provided by early agriculture, than is known about the lower limits. While it is agreed that the principle of exogamy is essential, limiting as it does competitiveness within closely cooperating groups, the question is how loosely this practice can operate in relation to post-marital residence and permanence of band affiliation. I have suggested that there are good reasons for tolerating far more ambiguities in the uncertain north woods than is possible for people with larger more permanent residence groups, who rely on clear unilineally defined and mutually exclusive divisions of the population for handling social-economic relations, and who marry out of the kin group, but not necessarily out of the local group.

However, the exogamy, cross-cousin marriage, and related terminology of eastern and northern Labrador raise the question: are these remnants of formerly existing clans? A similar question is raised by the uneven distribution of matrilineality, cross-cousin marriage and exogamy, among western Athabascans. Are they the result of Northwest Coast influence,

as is generally felt, or evidence of a previously more formal level of organization throughout Canada? Or, to return to Labrador, do cross-cousin marriage and exogamy represent 'incipient' clan organization, socially desirable alternatives which afford the basis for clans? In his discussion of cross-cousin marriage among Algonkians generally, Eggan points out that its essence is to "create multiple bonds between a limited group of relatives and maintain these from generation to generation, rather than tying nonrelatives together in an expanding system" (Eggan 1955: 532). He goes on to say that this form of residence tends to intensify local relationships at the expense of cross-group ties. However, when combined with local group exogamy, it strengthens both internal and external ties, just as the fully developed clan does. It is interesting, as Eggan mentions, that cross-cousin marriage is found scattered through the Great Basin and often among the more isolated groups (Eggan 1955: 537).

Should further ethnohistorical, archaeological, and linguistic research happily combine to clarify the nature of pre-Columbian social organization among Canadian hunters, the question would still remain: where do they stand in their historical relation to hunting-gathering societies in other parts of the world? The Athabascan and Algonkian hunters, who inhabit the vast interior of North America, far from the centres of higher culture, provide excellent candidates for 'pure' marginal cultures. There is at best limited influence from settled peoples on their peripheries; there seems to be nothing indicating an earlier horizon of denser population like that in the Eskimo area; nor were they pushed into marginal regions by advancing higher cultures in the same sense, say, as the once widespread Bushmen. Furthermore, the extreme marginality of the north woods economy, with starvation periods as a recurring danger, contrasts even with that of the Kalahari Desert. The Bushmen may have to tighten their belts in the dry season and forego having as much meat as they would like, but they can rely on the vegetable foods, which form the greater part of their diet. Certainly the uniquely elaborated definition of marriageable partners among the Australians was made possible by the relative dependability, predictability, and stability of the major part of their food supply, in contrast to the uncertainty of weather conditions and animal movements and distribution in the north.

At first, the marginality of the Canadian hunters might seem to make them good models for more 'primitive' or 'earlier' forms of social organization. Yet a moment's thought makes it clear that this is not the case. Their extreme dependence on meat, albeit supplemented by fish, and, in the short summer, some vegetable food, represents a late and specialized adaptation in the evolution of man, so far as we can tell from our present knowledge.³ Thus, from the long historic view, we cannot say that the ancient forbears of today's Canadian hunters did not have more formal organizational patterns adapted to a more sedentary foraging-hunting economy, patterns which were lost in adjusting to northern conditions, either in the Old World or the New World.

Although we may conclude with more questions than answers about Montagnais-Naskapi band organization, some things can be said with certainty. I have spoken of the wide latitude afforded the individual for choice of movement and group affiliation and have suggested it is not a

recent breaking down of a structure but an old adaptive pattern that takes place within a structure, one that, in fact, enables the structure to exist. (Indeed, it is not so different in kind, although perhaps in degree, from the flexibility found among hunting-gathering peoples generally, a good example of which is given by Hart and Pilling's discussion of the Australian Tiwi) (Hart and Pilling 1962). In any case, it is far from a simple matter. Albeit maddening to the field worker in its lack of formal definition, it involves a highly sophisticated process of evaluating population composition and territorial resources, and making one's choices both in relation to them and to personal preferences of oneself and others. This much, at least, is very clear: the 'individualism' this both necessitates and enables is far from the 'atomism' said to characterize the northeastern Algonkians. Seventeenth century material reveals that minimal units consisted of not one, but several nuclear families, that several such units cooperated closely much of the year, and that these larger groupings joined with others in a constantly fluctuating cycle of seasonal movement, aggregation, and separation. Recent field studies indicate that in-group cohesion and out-group ties are strengthened by exogamy and a tendency to cross-cousin marriage, and both early and late data indicate a strong matrilineal emphasis. This is the structure within which individual decisions are made. If their range is wide, it is necessitated by the conditions of north woods life and is only made possible by complete interdependence or the 'total' sharing of available resources.

NOTES

1. For those interested in space orientation, it is noteworthy that Mathieu oriented his maps around interior water sheds. As a result, in the first one he drew, the coastline became quite distorted. Later, after looking at my map, which at least gave the Coast in full detail, he corrected his mistake.
2. Collective ties have however been very persistent. It is interesting to note the correction I must make in my statement that the multi-family groups of south-eastern Labrador I felt to be closely related to early units existed by contrast with the nuclear family units recorded in the literature for the west, such as among the Mistassini and Lake St. John Indians (Leacock 1954: 21-2). When Rogers worked more intensively with the Mistassini on this problem, he found that nuclear families still work together in cooperative units or "hunting Groups," and some still occupy communal dwellings (Rogers 1963: 54-5). He also notes the problem these groups are faced with in the conflict between cooperative hunting traditions and the increasing economic independence, as a result of the individual ownership of furs by the nuclear family (Rogers 1963: 54-5, 68).
3. Though decisive steps toward humanity, taken by foraging primates in warmer climes, may well have involved cooperation and tool using in order to add meat to the diet, none the less foraging apparently continued to furnish the main bulk of subsistence, in the opinion of some, until Upper Palaeolithic times.

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DISCUSSION

ROGERS: There seems to be considerable diversity in the structure of the so-called 'bands' westward from the Atlantic toward Lake Winnipeg. I am amazed at this diversity in the southeastern area. Is there a possibility of a late movement of these people, whom we refer to as Montagnais, after the Eskimo were pushed out of the area? I wonder if this unsettled condition accounted for the flexibility and mobility that you have mentioned?

LEACOCK: The early *Jesuit Relations* report tremendous variability. If we examine the history of a particular band, considerable movement is indicated. Speck's material also indicates movement. The movement is being constantly reduced because of the presence of the trap-line. Once an Indian has laid down a line of 400 traps he has made an investment in an area, but even in such cases the extent to which the Indians will be more casual about traps, compared to the white trapper, is amazing. Even in the Seven Islands area, which seems to be an old area of stable trapping, the situation is still very fluid. We can still see this process of increasing stability developing around the trap-line. I do not think there was any evidence that there has not always been considerable movement.

ROGERS: It should be noted that Indians moved into the area rather late.

LEACOCK: That is true; in some areas they moved the Eskimo out. However, it was not true in the interior, for the Indians were not late arrivals in the Davis Inlet and Barren Ground areas where Strong worked.

HELM: I think that a similar situation existed in the Mackenzie area. The people who are known today as the Dogrib and the Hare moved down the river, but there was turmoil with the Cree who were directly to the south, and some of the people, known as the Slavey, seem to have been displaced. No one knows just how long the Hare have lived above the Arctic Circle.

LEACOCK: In other words, the idea that there was once a stable society that deteriorated must be rejected.

HELM: In the contact situation in which groups of 'two removes' are causing turmoil and ferment to tribesmen that have not yet had contact with the white man, the tribes pushed against one another. It is a question of whether they were in an area 100 years or 500 years before the final direct contact with whites. The archaeology of northeastern Canada is probably as scant as that of northwestern Canada and thus cannot, at this time, illuminate the problem.

DAMAS (to Leacock): Could you tell us something about leadership in the Naskapi-Montagnais area?

LEACOCK: There is a distinction, which I have mentioned elsewhere, between the inside chief and the outside chief. The outside chief is the elected chief of the band and is really the go-between for the band and the Indian agents. He is often considered to be a spy for the whites. This is

obviously a recent introduction; otherwise chieftainship is decentralized. The inside chief is only a chief temporarily for a particular hunting party.

DAMAS: With regard to the periodic assemblages of the earlier period to which you referred in your paper, were ceremonial functions involved?

LEACOCK: Only to a minor extent, although there certainly was festive activity at the gatherings. After the coming of the missionaries, baptisms and marriages also took place at these assemblages.

HELM: Do I understand that the assemblages were fisheries? How did they support themselves during the period of aggregation?

LEACOCK: There probably was fishing in the earlier period as is noted in the *Jesuit Relations*. Today the assembled Indians live mainly on flour.

EGGAN: Did the summer–winter variation in group size and dispersal have an ecological background in the aboriginal period? You seem to have destroyed the ecological background for the family hunting territory.

LEACOCK: There is a very important ecological aspect in their coming out of the woods in the summer.

EGGAN: All throughout northern North America and also through most of aboriginal North America there was some seasonal shift from small to larger groups except in the intensive agricultural areas. The interior Eskimo are especially interesting because there the shift occurs in seasons opposite to those of the rest of the area. One thing that we should keep in mind regarding such shifts is the question of whether there is an economic advantage to this pattern of fusion and fission or whether there are purely social or ceremonial reasons involved. We must ask the question: what would happen if the pattern of fusion and fission did not take place? In the Plains it is very clearly an ecological problem. The Cheyenne tried to stay together during the winter one year, and they nearly starved to death. Would this happen in the northern areas as well?

HELM: My guess would be that in the Déné area the festive activities like the hand game and dancing occurred at the time of the fisheries. Perhaps two or three hundred people would aggregate and would dance "until you couldn't see the other side of the circle." Meetings of potential mates occurred at these gatherings as well, but all of these things got accomplished in an ecologically favourable setting; that is, the fisheries were the reason for the aggregation.

BICCHIERI: I have written about the problem of aggregation for the BaMbuti of the Ituri forest. There are two chief groups, the net hunters and the archers. The net hunters join in regular groupings during the cooperative net hunting seasons. They seem to use the occasional periods of abundance of honey to break up into smaller groups so that they can reconstitute partnerships and get away from people with whom they are not congenial. The archers, on the other hand, who are more scattered during the year, use the time of honey abundance not only to lessen tensions but also to

establish relationships, each according to the particular pattern in which the respective group normally lives.

HELM: Perhaps we cannot make a distinction between these motives. What we must consider is whether there was ecological opportunity that makes aggregation feasible for a month or so. Do they take advantage of the ecology in order to re-establish some sort of social identity beyond the fragmenting and coalescing small group of people?

BAND ORGANIZATION AMONG THE INDIANS OF EASTERN SUBARCTIC CANADA

By EDWARD S. ROGERS

RÉSUMÉ

L'étude qu'on va lire traite de la structure et de l'organisation des bandes telles qu'on les rencontre chez les Cris-Ojibways de la zone subarctique de l'Est du Canada. La première difficulté qui se présente consiste à déterminer ce qu'est exactement une bande. Il en fut proposé plus d'une définition dans le passé. On peut suggérer certains paramètres grâce auxquels il deviendrait possible d'isoler la notion de bande. On examine ensuite la bande telle qu'elle existe chez les Ojibways du Lac Rond et chez les Cris de Mistassini en fonction des paramètres spécifiés. Toute la région habitée par les Cris-Ojibways de la zone subarctique de l'Est est alors envisagée du point de vue historique. On propose enfin une définition de ce qui est considéré comme une bande.

INTRODUCTION

The subject of this paper is 'band' structure and organization among the Cree-Ojibwa Indians of Eastern Subarctic Canada.¹ These people lived in isolated groups in an area extending approximately from the Atlantic coast of Labrador, west to Lake Winnipeg and Northern Manitoba, north to Hudson and James bays and Hudson Strait, and south as far as the St. Lawrence and Ottawa rivers, and slightly north of the Great Lakes (Map No. 1²). These few scattered Indians exploited a harsh environment with a limited technology, which was employed in hunting, fishing, and trapping.

A typical continental climate prevails throughout the area. The summers are short and relatively warm, the winters long and at times intensely cold. The land, though forested primarily with black spruce, sustains white spruce, balsam fir, poplar, and birch. A network of lakes, streams, and rivers unites the entire area, and interspersing these waters are extensive areas of muskeg. This country is all part of the Precambrian Shield.

Although there is a basic uniformity throughout the area, natural resources vary and have varied slightly from region to region as a result of environmental conditions and historical circumstances. Formerly, woodland caribou ranging in small herds were generally important except in the Hudson - James Bay Lowland where waterfowl were extensively exploited. Everywhere, fish, although limited in number, were important for subsistence. In time, however, the number of caribou decreased and the significance of small game such as hare, and of moose, when they entered the region, increased (*see* Rogers 1964). Throughout most of the contact period, the fur trade was a vital factor in the lives of the Cree-Ojibwa. Their way of life has been altered drastically, and today they are linked intimately with Euro-Canadian society.

Before 'band' organization and structure can be discussed, the concept of 'band' must be considered. For many years the term 'band' has been used

to designate groups of people inhabiting the Eastern Subarctic and other areas of the world. It has been applied to small social units with weakly developed political organizations within which the people subsist by hunting, fishing, and (or) gathering. Although various definitions have, at one time or another, been given, rarely has much thought been devoted to determining exactly what is meant by the term 'band.' There are those definitions proposed by ethnologists who have carried out field-work within the Eastern Subarctic, and by those more interested in a theoretical approach to the concept of 'band' and its implications for the evolution of society.

Among Eastern Subarctic field workers, Speck, the pioneer ethnologist in the region, was the first to define 'band,'

... a group inhabiting a fairly definite territory with more or less stable number of families, possessing paternally inherited privileges of hunting within tracts comprised again within boundaries of the territory. Often having an elected chief, speaking with idioms and phonetic forms by which they and outsiders distinguish themselves as comprising a unit, often with minor emphasis on this or that social or religious development, often with somewhat distinctive styles of manufacture and art, and finally, travelling together as a horde and coming out to trade at a definite rendezvous on the coast. (Speck 1926: 277-8)

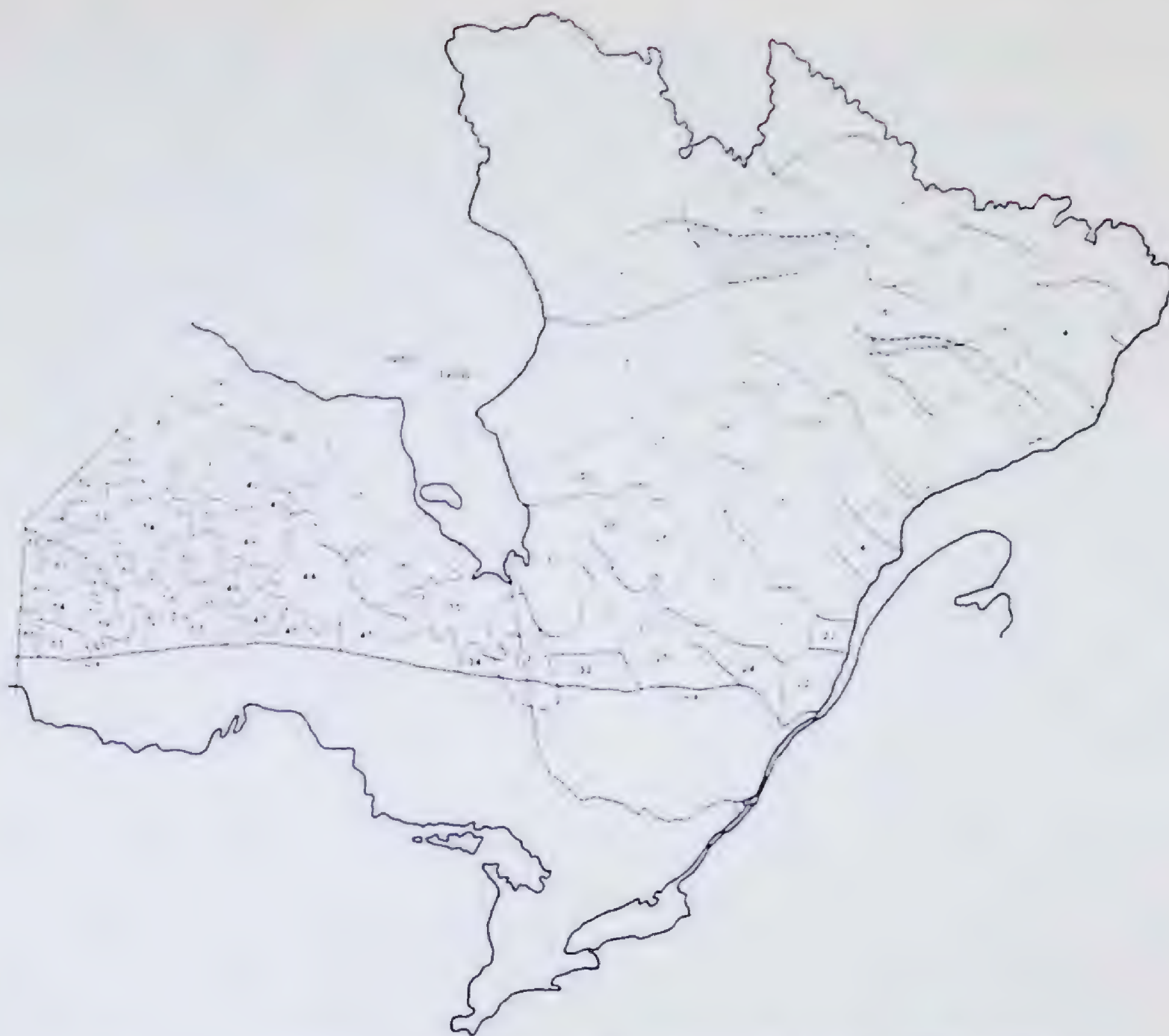
Honigmann, although giving no formal definition of the term 'band,' refers to the largest social unit among the Montagnais-Naskapi of the Labrador Peninsula as a macroband. The macroband was composed of a number of microbands (hunting groups), each of which might contain as many as fifty persons. He felt that in pre-contact times, the macroband rarely assembled as a complete unit where everyone met face-to-face. Later, segments of macrobands were drawn together at convenient trading localities, especially when there were no trading posts within their home territory. In time, these gatherings became formalized administrative units for government purposes (1964: 332).

Leacock, like Honigmann, does not give a formal definition of the 'band.' She states, in the case of the Indians of the southeastern portion of the Labrador Peninsula, that "band territories in this area have become stabilized only in the last generation." She continues, "As to band membership, there is no evidence to indicate that it has not always been in constant flux" (1954: 20). McGee concurs, when speaking of the Indians of the Northwest River, "Both band structure and composition appear to be as fluid today as it was in 1868" (McGee 1961: 31).

Dunning wrote of the western part of the Eastern Subarctic, "Generally speaking, the band represents the total society from the standpoint of one of its members. Even within recent years the total number of persons in the society was small" (1959: 54).

One of the first of several ethnologists who attempted to place Algonkian 'band' structure in world perspective was Steward. He termed what he believed to be the maximal social unit among the Algonkians of the Eastern Subarctic as the "composite band." He described this unit as follows:

Like the patrilineal band, it is politically autonomous and controls the principal resource in its hunting territory, but it is much larger than the patrilineal band and hence lacks band exogamy, patrilocal residence after marriage, and



MAP 1 — *Distribution of Bands in the Eastern Subarctic 1900-1960.*

Bands in the Eastern Subarctic 1900-1960

(1) Barren Ground	(23) Tadoussac	(45) Pagwa
(2) Davis Inlet	(24) Lake St. John	(46) Nakina
(3) Northwest River	(25) Tete de Boule	(47) Auden
(4) St. Augustine	(26) Mistassini	(48) Fort Hope
(5) Musquaro	(27) Neoskweskau	(49) Lansdowne
(6) Natashkwan	(28) Eastmain	(50) Big Beaver House
(7) Mingan	(29) Nemiscau	(51) Sachigo
(8) Michikamau	(30) Rupert House	(52) Island Lake
(9) Ungava	(31) Waswanipi	(53) Sandy Lake
(10) Petisikapau	(32) Grand Lake Victoria	(54) Round Lake
(11) Moisie	(33) Abitibi	(55) Pickle-Crow
(12) St. Marguerite	(34) Lowbush	(56) Osnaburgh
(13) Shelter Bay	(35) Moose Factory	(57) Sioux-Armstrong
(14) Godbout	(36) Albany	(58) Sioux-Hudson
(15) Bersimis	(37) Attawapiskat	(59) Lac Seul
(16) Kaniapiskau	(38) Winisk	(60) Cat Lake
(17) Great Whale River	(39) Fort Severn	(61) Deer Lake
(18) Fort George	(40) Bearskin	(62) Little Grand Rapid
(19) Nichicun	(41) Big Trout Lake	(63) Pikangikum
(20) Lake St. John	(42) Casabanica	(64) Red Lake
(21) Chicoutimi	(43) Webique	(65) Grassy Narrows
(22) Escoumins	(44) Ogoki	(66) Islington

patrilineality. It consists of many unrelated families which may intermarry within the band. (Steward 1958: 144)

Later, Service reviewed 'band' structures on a worldwide scale in his book *Primitive Social Organization*. He did not agree with Steward that the 'composite band' was the original type to be found among the Algonkians of Eastern Canada:

If we cannot conclude with finality that the aboriginal Algonkians were typical patrilocal bands in all respects, it does make the best supposition. . . . The unorganized, fluid, composite band of more recent times is readily accounted for as a consequence of the coming of the Europeans. (Service 1962: 86)

Service defines the two types of bands, the first: "the patrilineal band (this is the same as the patrilocal band) is a group which is exogamous and virilocal"; the second type: "the composite band is one which lacks exogamic rules and explicit marital residence customs" (Service 1962: 60).

The above indicates that there are certain disagreements about what constituted Eastern Subarctic 'band' structure. Furthermore, the definitions, if they may be called that, tend to be exceedingly vague in many cases and in others, perhaps, too rigid.

Before it is possible to examine the data relating to 'band' organization and structure, certain, perhaps interrelated, parameters which might delimit a 'band' must be specified. These parameters are as follows: Do named groups exist? And, if so, what makes up the composition of such a group? What defines the boundaries of the territory inhabited? Is there any cultural homogeneity within such a group that distinguishes it from other groups? What are the relations between one such group and similar groups? What is the process of fission and fusion within such a group?

It must be remembered that the Cree-Ojibwa have been under Euro-Canadian contact for as long as three hundred years. There was the period of initial contact about which little is known, followed by a time of involvement in an active fur trade economy. Finally, there is the present, spanning approximately the past twenty years, when the Cree-Ojibwa have been subjected to the full impact of and became an integral part of Euro-Canadian society. This culture contact must constantly be remembered when dealing with the problem of 'band' organization and structure. In this paper two groups, the Round Lake Ojibwa and the Mistassini Cree, are dealt with in detail, and an overall view of all data for the Eastern Subarctic is given.

THE ROUND LAKE OJIBWA

Field-work was conducted among the Round Lake Ojibwa of Northern Ontario during a period of one year, from 1958 to 1959 (Rogers 1962).

Today the Round Lake Ojibwa inhabit a village on the shores of Weagamow Lake for a part of the year.³ These people have not always gathered in this village, but in 1949 a Hudson's Bay Company trading post was built at this location and became the focal point for their summer aggregation. Since then, a church, a school, and other facilities have been established by Euro-Canadians within the village for the benefit of the Indian people. The presence of these establishments has encouraged them to remain there during the summer and, recently, during other parts of the year as well.

The following analysis based on informants' testimony covers a period preceding intense contact extending from approximately 1900 until the establishment of the trading post at Weagamow Lake. Formerly, at least five named groups inhabited the present territory of today's Round Lake Ojibwa,⁴ the *atihkopi · wininiwak* (Caribou Lake People), *wa · wiye · kama · wininiwak* (Round Lake People), *wi · ntiko · wininiwak* (Windigo Lake People), and *mahkopi · wininiwak* (Makoop Lake People). The fifth group, the *oci · kosa · kahikani · wininiwak* (Fisher Lake People), appears at times to have been an independent and autonomous unit and at other times to have been merged with the Windigo Lake People. Each of the above groups gathered at a particular lake within their respective territories (the lakes which gave them their names). In the fall they radiated from the lake in hunting groups to wintering areas and returned in the spring.

Although these were the proper names by which the people knew each other, there was a general term for such a grouping, *nintipe · ncike · win*, translatable (from a leader's point of view) to "That which I am in charge of." Although this term was used by the people of themselves, alien Indians had a separate term, *oîcacosic*, covering four of the five groups. It referred to the Round Lake, Caribou Lake, Fisher Lake, and Windigo Lake peoples as a unit.

This large grouping was quite unstable and, accordingly, is of no further concern in this discussion. The smaller unit, referred to as *nintipe · ncike · win* is, however, of importance here. The problem is to examine this group in terms of the parameters specified previously.

What was the composition of such a local, named group? The composition of two of these groups spanning five generations or approximately one hundred years is given in the following tables:

TABLE 1
Caribou Lake Group

- 7 Families
 - 67 Married men
 - 29 Men remained within the territory.
 - 4 Men moved to the territories of other groups.
 - 92 Wives
 - 32 Wives were from the local group.
 - 20 Wives came from neighbouring groups.
 - 50 Women born within this group
 - 24 Women remained after marriage.
 - 20 Women, after marriage, moved to another group.

	Bands					
	Big Trout	Round Lake	Big Beaver House	Windigo	Bearskin	Pickle Lake
Foreign wives from:	3	7	2	7	1	0
Caribou Lake women moved to:	2	9	6	0	2	1

The Round Lake People exhibited a similar movement of individuals:

TABLE 2
Round Lake Group

- 7 Families
 - 37 Married men
 - 23 Men remained within the territory.
 - 9 Men moved to the territories of other groups.
 - 46 Wives
 - 15 Wives were from the local group.
 - 19 Wives came from neighbouring groups.
 - 22 Women born within this group
 - 7 Women remained after marriage.
 - 15 Women, after marriage, moved to another group.

	Bands							
	Caribou	Big Beaver House	Windigo	Bearskin	Sandy	Fisher	Cat	Winisk
Foreign wives from:	9	2	4	1	1	1	0	1
Round Lake women moved to:	6	0	5	1	0	2	1	0

It is clear, from the above figures, that the men tended to remain within the territory of their natal group.⁵ In the case of the Caribou Lake group, four males left and three entered the territory.⁶ There was a much more extensive movement of women: twenty women left and twenty from surrounding groups entered the group. The Caribou Lake people most frequently intermarried with Round Lake and Windigo Lake peoples. Among the Round Lake people who generally intermarried with Caribou and Windigo Lake peoples, 9 men and 15 women emigrated and 19 women came from surrounding groups. Accordingly, these local, named groups were neither strictly exogamous nor endogamous, but they did have a patrilocal bias (see Figures 1 and 2). Also the local, named group did not become a patrilineal kin group. Each local, named group was composed of in theory several unilateral but in practice bilateral extended families, and each was referred to as *nintipe · ncike · win*. This group, consisting ideally of a group of males and dependent women who were patrilineally related and limited in depth to three generations of males, was an extremely important social unit in the life of these Ojibwa. It was important during the summer when all members were in face-to-face contact. It might operate during the winter, if it operated as a hunting group. The members were controlled and dominated by the eldest male. Such units formed 'cooperative associations' and stood in opposition to other such units with which they exchanged females. The *nintipe · ncike · win* allocated females in marriage and took them back on the deaths of their spouses. It was a unit based on kinship rather than residence,

although the two might be combined. Several of these units composed the local, named group.

Feuds between such units arose, primarily over the residence of the woman and her spouse. Perhaps a man had few or no sons and wished his daughters to remain with their families, bringing in their husbands. A factor of some

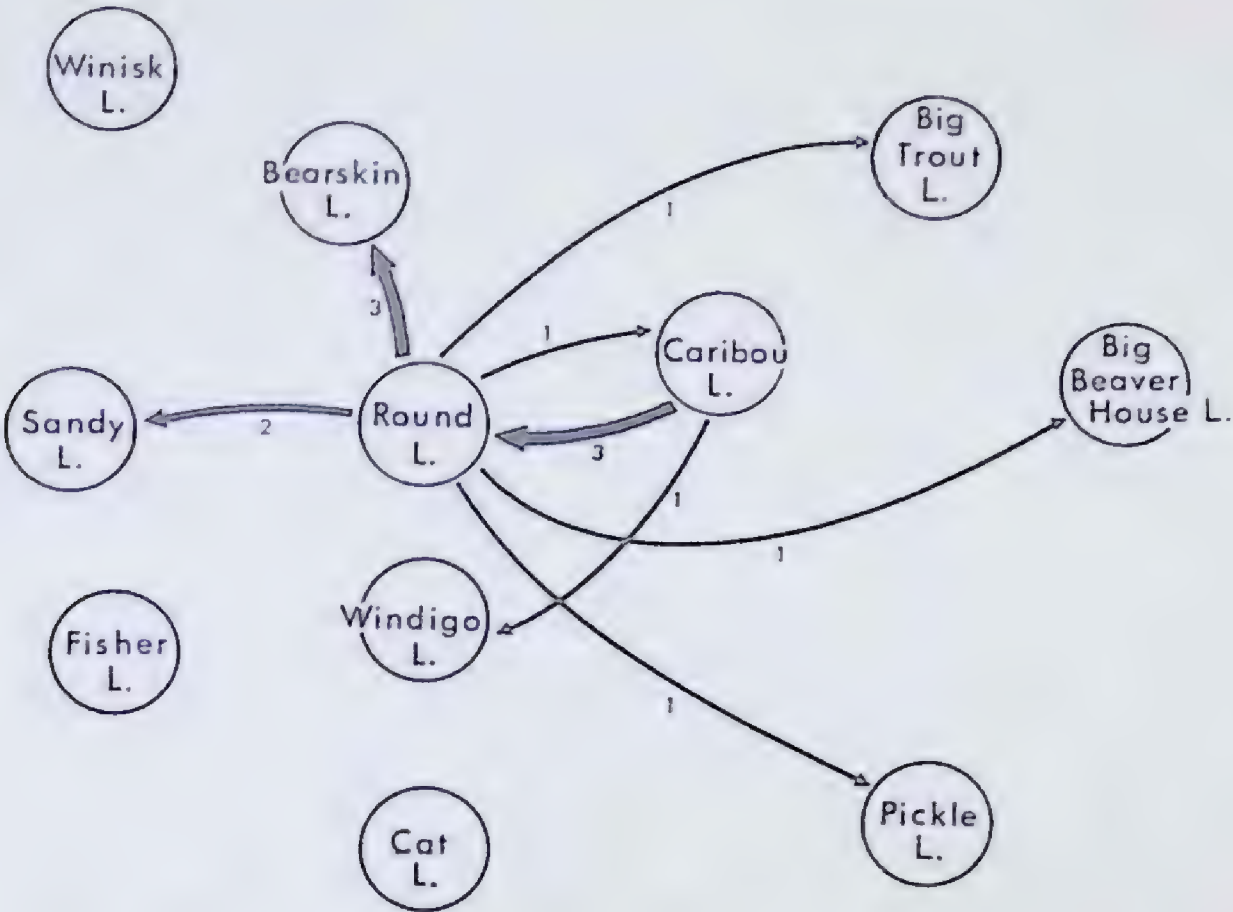


FIGURE 1 — Movement of Men after Marriage 1875-1958.

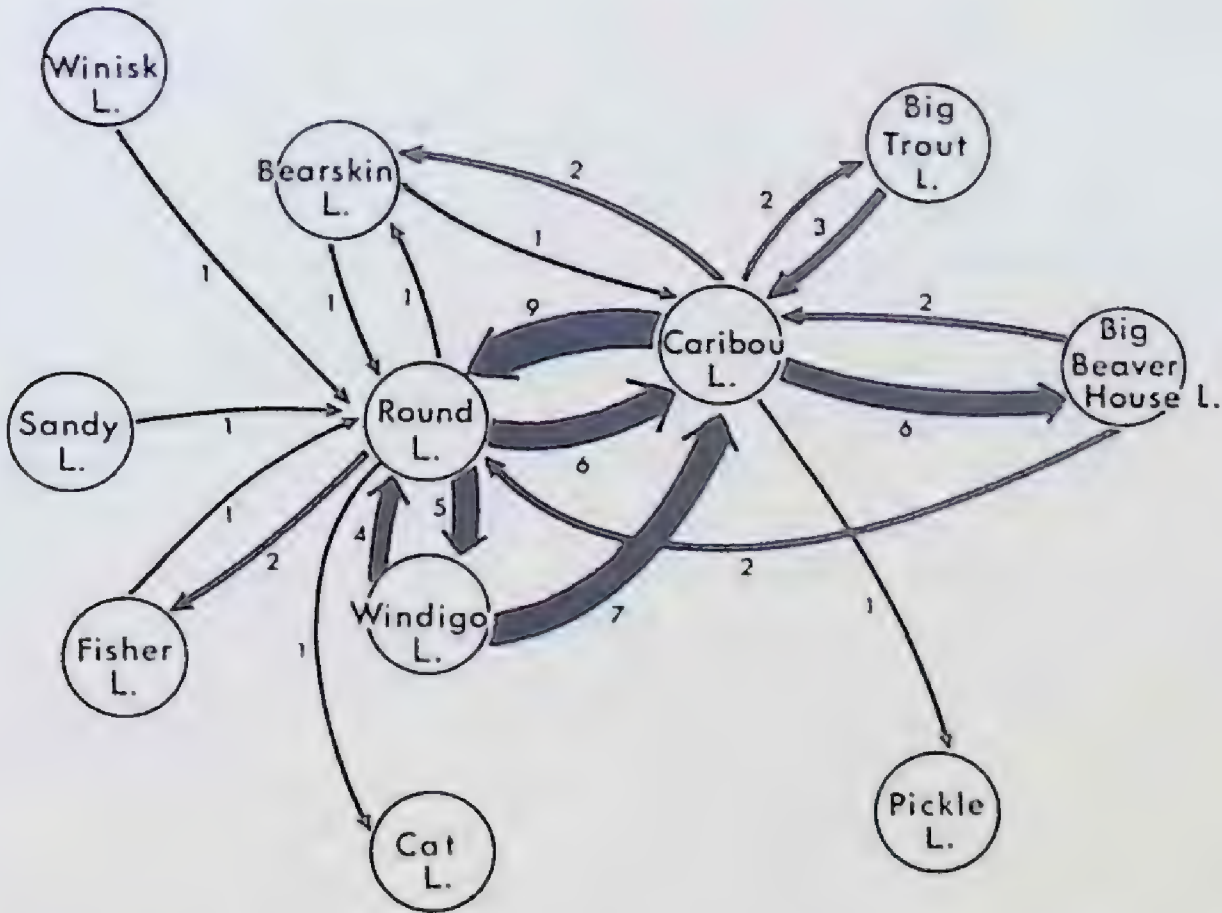


FIGURE 2 — Movement of Women after Marriage 1875-1958.

importance in alleviating the resulting opposition and tensions between *nintipe · ncike · win* of particular males was bilateral cross-cousin marriage. Since the two fathers were brothers-in-law and therefore ideally close friends, the tendency to engage in a feud was less than in the case of leaders of two unrelated *nintipe · ncike · win*. All cases of trouble between fathers of married couples that were described by informants occurred between unrelated males. However, the most serious disagreements took place between men and their otherwise unrelated fathers-in-law.

An analysis of one Caribou Lake family, over a number of years, showed that 35 per cent of the wives came from surrounding local, named groups while 65 per cent came from within the local, named group. Almost half of the latter were wives who were related, although often only remotely, to their spouses in one way or another. The practice of cross-cousin marriage made it possible for unions to take place between extended families and within the extended family group. This practice was encouraged since it bound related nuclear families more closely together.

It can be inferred from the above that each local, named group was composed of a core of males related primarily consanguineally but occasionally affinally. In addition, there were the unmarried females and children and wives who might, or might not, have originated within the home territory.

The next factor to be examined is the operation of the local, named group. Generally it functioned independently except for marriage arrangements and a few other ties. In order to gain a livelihood in this rather harsh environment, the people remained scattered for most of the year, only congregating, as a rule, during the summer months. At this time they met at a convenient fishing locality on the shores of one of the major lakes, such as Caribou, Weagamow, or Windigo. Not all families assembled each year at the same lake, but occasionally individual families visited relatives at another summer encampment. Fishing was important during this period. At the same time, hares were snared, and waterfowl and grouse were secured. Near the end of summer, berries were collected.

At the close of the summer season, the people dispersed in hunting groups to their particular hunting areas. The hunting group was based on residence and economic cooperation and coincides with Dunning's "co-residential group" (1959: 57-8). At the same time it might also be secondarily based on kinship. If this was the case, it then was the same as the extended family. Each group was relatively small, numbering perhaps ten to fifteen people, which comprised two to four closely related families, and was under the direction of the eldest male. During the fall, the groups camped near favourable fishing rapids where traps were built to secure large quantities of whitefish to be dried for the months ahead. Subsequently each group moved, as a rule, to a favourable hunting locale, and occasionally the groups met after freeze-up for a feast. At the end of December they reunited at the summer encampment, where the leader of the local, named group usually had spent the fall and early winter, for a few weeks of general festivities.

Following this reunion the people again dispersed in search of game and fish. Since few fish could be taken at this time of year, each hunting group was constantly on the move leaving the old people and women in camp to fish while the men searched for game.

Just prior to the arrival of spring, each hunting group moved to the place where the canoes had been left the previous fall. With the coming of spring, subsistence was much easier. Fishing was highly productive, and large numbers of waterfowl arrived from the south. Immediately after breakup was complete, the hunting groups returned to the summer encampment where the winter's success was celebrated with a feast. Thus another yearly round began.

Informants stated that there was a leader, or *okima*., for each local, named group (see Ellis 1960: 1). His position was neither hereditary nor elective. Rather, he was a charismatic individual who was believed to have superior religious power, which was directed toward protecting the group from harm. A leader contributed goods to the group, cared for the sick and orphaned, and settled internal group differences. He also organized feasts that were held at regular times of the year. Informants stated that each group had its own individual leader. In one instance *ki · hci* David unified several local groups under his leadership. Informants could recall the names of these leaders for a period covering perhaps seventy-five to a hundred years (Figure 3).

It has been stated that for a large part of the year the people remained scattered in hunting groups throughout the territory of the local, named group. Much has been written regarding the institution of hunting territories as it existed among the Indians of the Eastern Subarctic. Suffice it to say that the Indians referred to in this paper as the Round Lake Ojibwa formerly had 'hunting areas' to which the hunting groups returned every year (Rogers 1962: C22-C24, 1963). Nevertheless, all resources were free goods, and anyone had the right to exploit them. Accordingly, there could be shifts from year to year in the areas where individuals or families spent the winter. It was these hunting areas that defined the territory of any one local, named group since they formed a continuous block of land. It is possible, therefore, to think of a local, named group as a number of hunting groups exploiting contiguous territories during the winter months and uniting at a favourable fishing locality on some major lake within this area during the summer months.

Another parameter to be considered is whether each of these named groups showed any tendency toward cultural homogeneity. Although there may have been minor diagnostic characteristics for each group, the data are at present insufficient to indicate this. Nevertheless, there were several characteristics which did distinguish the Caribou Lake, Round Lake, Windigo Lake, and Fisher Lake peoples.

First there were certain differences in the kinship behaviour among these four groups compared with those to the north and east, the Sachigo, Bearskin, Big Trout Lake, and Big Beaver House Indians. Among the former, the term for father's brother meant 'little grandfather,' and to the north, 'little grandmother.' In this northern area, there were certain parent-in-law tabus and cross-cousin joking relationships not found among the four groups resident in the Round Lake area. In addition to the slight variance in kinship behaviour, the people living in the region of Weagamow Lake did not possess the *manito · ki · wak* ceremony (Rogers 1962: D11), practised by peoples to the west at Sandy Lake. Finally, the four groups mentioned

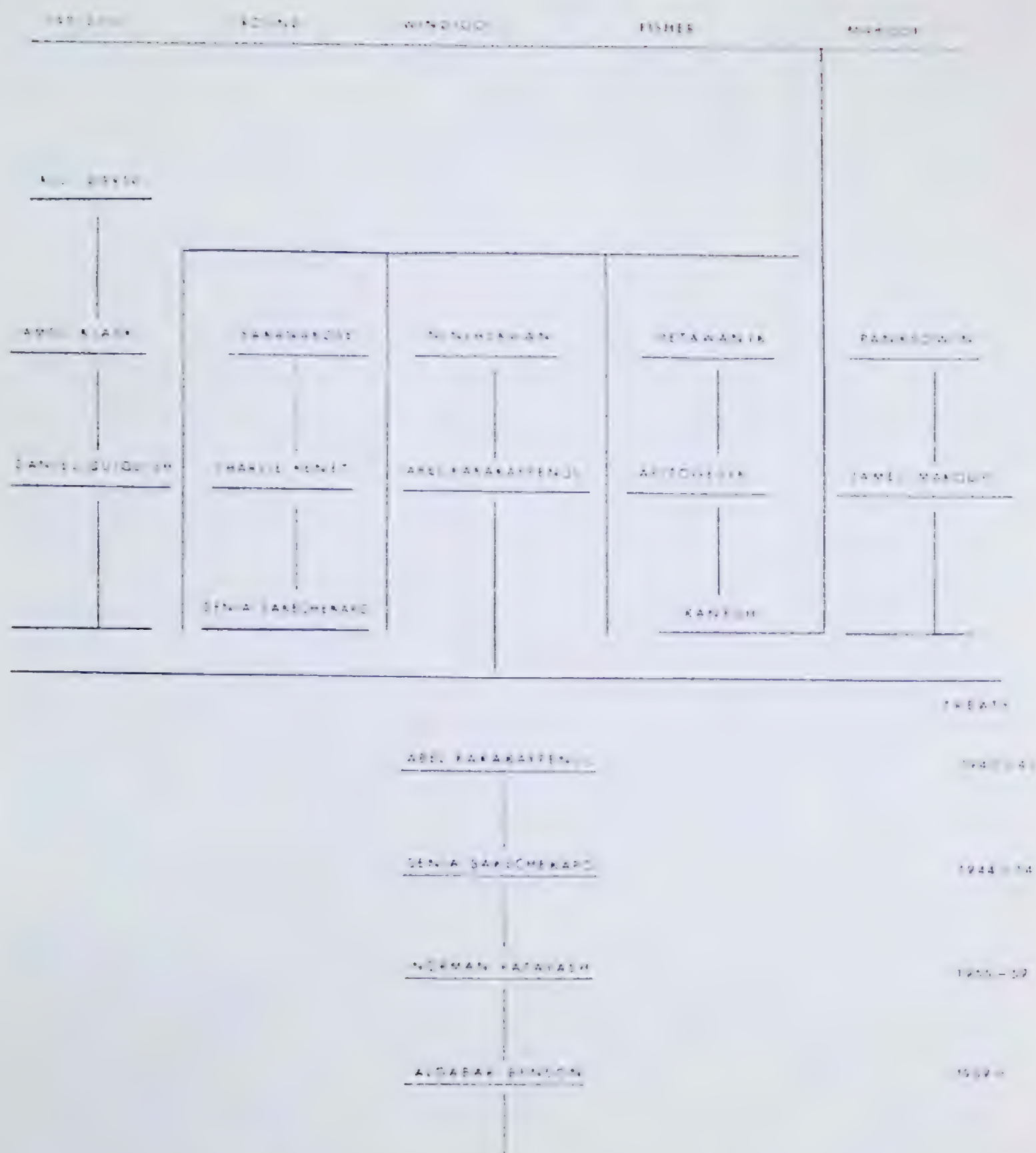


FIGURE 3 — Leaders Among the Round Lake Ojibwa.

above had no concept of clan organization nor had they similar practices, such as curing by means of a sucking tube, as are found in groups to the south.

The type of relationship which existed between these local, named groups is the next factor to be examined. It has been pointed out that a high degree of intermarriage took place between local, named groups. The movement was one of women rather than men. The simultaneous transfer of goods and ideas as well as women indicates a degree of communication between groups. For example, in 1945 the families living at Makoop Lake adopted the beaver snare from the Bearskin Indians, and from there its use spread

south to the inhabitants of the Round Lake area. In 1950, at the same time that the strut sled was introduced to the Round Lake area from Big Beaver House, the puckered mocassin was introduced from Bearskin. In 1958 the beaver net was introduced by way of Cat Lake to the Round Lake people. Previously, at unknown dates, other items of various origins had been adopted in the area.

Although the above suggests that friendly relations existed between local, named groups, this was not always the case. The people of the Round Lake area frequently had disagreements with those living at Sandy Lake, Sachigo Lake, Trout Lake, and Cat Lake. The troubles that arose, most frequently over women and trade goods, were dealt with through various forms of witchcraft. Warfare was not part of their cultural heritage. The five groups within the region of Weagamow Lake — Caribou Lake, Round Lake, Windigo Lake, Fisher Lake, and Makoop Lake — did not have serious disagreements.

Population density was quite sparse and no local, named group, as a rule, numbered more than seventy-five people and in some cases may have been as few as twenty-five individuals. These figures are based on present census data. The population has increased in the past few years (*see* Table 3), although informants contend that it is no larger today than it was fifty to sixty years ago.

The final point to consider is the extent of fission and fusion which existed among these local groups. Unfortunately, there are very little data regarding this matter. As mentioned previously, there was once a union of four local, named groups. However, no information exists as to how they operated under the direction of a single leader. In addition, the Fisher Lake and Makoop Lake peoples appear to have been subgroups, the former of Windigo

TABLE 3*

Year	Population	Area (sq. mi.)	Density (individual per sq. mi.)
Round Lake Ojibwa:			
1900	250?	5,000	1/20
1949	216	5,000	1/23
1959	268	5,000	1/19
Mistassini Cree proper†:			
1829	75-100?	30,000?	1/300
1914	187	30,000?	1/160
1952	450	30,000?	1/66

*Figures derived from Rogers 1962:B2, B7; Rogers 1963:5, Table I, p. 23, based upon Lips 1947:458-9; and Speck 1917:90 and field notes.

†In 1953, the combined total population for all groups that gathered at Lake Mistassini was 664, giving a density of approximately one person for every 66 square miles.

Lake and the latter of Round Lake, possibly Bearskin, peoples. It is conceivable that these groups were rather ephemeral and would, if their numbers decreased considerably, fuse with a neighbouring group. The group's proximity to a lake full of fish during the summer months and the abundance of game in the vicinity would seem to account for its existence at any particular

point in time. Witchcraft also may be a factor in group fission. Much more information is needed before even a tentative answer can be given.

THE MISTASSINI CREE

The second group to be considered is the Mistassini Cree of south-central Quebec, among whom field-work was undertaken during 1953-4. These people inhabited an environment quite similar to that found at Weagamow Lake, except that it is a much poorer area since game densities and fishing productivity are exceedingly low. Accordingly, the people were much more widely scattered throughout the area than they were at Round Lake. Table 3, giving population densities over time in the two areas, indicates this scattering.

Unfortunately, detailed information regarding band organization among the Mistassini Cree is much more limited than for the Round Lake Ojibwa. This is partly the result of the intensive contact they have been confronted with for the past three hundred years, which has greatly altered much of the old way of life. Because of this deficiency, it is necessary to fall back upon those historical sources available and to try to extract all possible evidence from them.

The Mistassini Cree were first mentioned by name in 1642-3 (Thwaites 1896-1901, 24: 155). Since that time there have been repeated references to them, but unfortunately the exact population unit encompassed by the term 'Mistassini' is not clear. At times it appears to include a relatively large number of people within an extensive territory and at other times a much smaller group inhabiting a smaller area. Within the general area, there were at different times various named groups. In 1820, the Indians to the northwest of Lake Mistassini were known as Neoskweskau and those to the southwest the Rush Lake Indians. Trading posts were located in both these areas, but by 1823 the Rush Lake Post was closed. Subsequently, many of the Indians began to trade at Mistassini and as a result became known as Mistassini Indians (Lips 1947: 454). At about the same time, the Neoskweskau Post closed. Those Indians who had previously traded there began to trade at the Mistassini Post. They were then listed as Mistassini Indians (Lips 1947: 455). At a later date a group called the Neoskweskau Indians again made their appearance in what Speck speaks of as northern Mistassini territory. They still retain that name. In 1825, the "Timmiskimay" Post was built northeast of Lake Mistassini (Davies 1963: 205, note 1), and those Indians who hunted along the Temiscamie River on which this post was established became known as "Timmiskimay" Indians (Lips 1947: 418). During the course of field-work, informants stated that the Temiscamie Indians, with the closing of the post, had joined with the Mistassini Indians. When, in recent years, the southern families of Speck's Mistassini began to gather about a new trading post at Lake Chibougamau, they were quickly recognized as a distinct group and named after the post. As a result of its closing a few years later, many of the so-called 'Chibougamau' Indians returned to Lake Mistassini as Chibougamau Indians, but at least one member of this group declared that he was a Mistassini Indian.

In 1958-9 all the Indians from Nichicun, Neoskweskau, Nemiscau, and Chibougamau who gathered about the trading post at the south end of Lake Mistassini were beginning to lose their identity as separate units. One infor-

mant even claimed that they should all be considered Mistassini Indians since they now gathered and traded at the same place.

The above suggests that the traders or missionaries never knew a group called 'Mistassini' with continuity of relatively stable membership for any great length of time. Trading posts, instead, tended to delimit the groups and at times gave the basis for their names. In fact, one informant clearly stated that the groups were named for the post where they traded. Nevertheless, this does not mean that before the advent of trading companies there were no summer gatherings of substantially the same hunting groups year after year and that these summer groups did not have names for themselves. In 1829-30 it was reported that Mewoppaish was the "principal" Indian of a small party that usually gathered a short distance east of Lake Albanel in summer and occasionally in winter (Lips 1947: 461). There was no trading post in this area. As McFeat (1962: 20) points out, trading posts merely established new foci for groupings and attracted larger numbers of Indians.

Although it is impossible to sharply delineate named groups, groupings of a sort existed and a few general comments can be made regarding their composition. The available information indicates that residence was ideally patrilocal within the local, named group and hunting group. It is possible to trace male ancestors for three generations as having occupied the same hunting areas. Nevertheless, there was a rule of temporary matrilocal residence during the first three years of marriage. This is an old pattern in the area and was reported as early as the 1820's (Lips 1947: 431, 460, 466).

Marriage tended to occur most frequently between members of adjacent hunting groups. This practice was encouraged by cross-cousin marriage, which seems to have been the preferred pattern. Nevertheless, taking the Chibougamau, the Neoskwekau, and the Mistassini Indians proper as a unit, slightly more than twenty per cent of the ninety-six marriages recorded, where the origin of the wife was known, took place with individuals from points as distant as Lake St. John and Rupert House. Over half of these marriages were contracted with women living to the west at Nemiscau or James Bay, four with Waswanipi women, three with Nichicun women, and one with a Lake St. John woman. About eighteen per cent of the women of the Mistassini-Chibougamau-Neoskwekau group married men from other groups, half of whom were from Lake St. John, four from James Bay, and three from Waswanipi. In the 1820's and 1830's, a somewhat comparable situation seems to have existed (Lips 1947: 417). Intergroup marriage, especially with James Bay people, may be the result of the employment of Mistassini on the fur brigades which travelled to Rupert House each summer until the 1920's.

Several cases of men as well as women moving at marriage were reported at the time of field-work; a similar situation existed in the 1820's to 1840's (Lips 1947: 457, 458-9, 462, 463, 467). However, it appears that men moved much less frequently than did women. During the past fifty years, outside marriages appear to have been decreasing in frequency.

Information about leadership patterns among the Indians of the Mistassini region is somewhat conflicting (*see* Rogers 1965). Lips reported the

occurrence of strong chiefs whose qualifications included high ethical standards, a consciousness of community responsibility, and, above all, hunting ability. If a chief failed in these requirements, perhaps because of advanced years, he was disregarded. Furthermore, a chief was expected to maintain peace, represent the interests of the group in transactions with outsiders, deal with violations of hunting rights within the band, redistribute unoccupied land, arbitrate in domestic affairs, aid those in need, force an individual to clear his debt by selling his furs to the Hudson's Bay Company, and lead his people in war against the enemy (Lips 1947: 401-4). It is to be noted that there is no record of warfare ever occurring in this area. A chief passed sentence in private except in cases of major offences when he depended upon the verdict of a council (Lips 1947: 403-4).

This characterization of the chief's duties probably applies primarily to the latter part of the last century at the height of the fur trade and not, as Lips contended, to an earlier period for which there is no adequate documentation regarding the duties of the chief. As LeJeune said in 1634:

... all the authority of their chief is in his tongue's end; for he is powerful insofar as he is eloquent . . . he will not be obeyed unless he pleased the Savages. (Thwaites 1896-1901, 6: 243)

Furthermore, there is no evidence of one leader for all the people who, in the past, have been called 'Mistassini.' Rather, several men were simultaneously referred to as 'chiefs.' In 1671-2, Albanel spoke of numerous "chiefs" (Thwaites 1896-1901, 56: 215), and Crespieul in 1673-4 gave a similar impression (Thwaites 1896-1901, 59: 43). Formerly, local groups were small and may have constituted *ad hoc* political units, headed by prominent men, often referred to in the early literature as 'chiefs,' 'captains,' or 'principal men.' These men, whose positions were probably earned on the basis of kinship, age, and wisdom, appear to have had little power outside their own extended families; they can be considered *primus inter pares*.

Are there any cultural traits which might set apart local groups in the region of the Mistassini? Unfortunately, information is meagre on this point. Mistassini informants did contend that the Nichicun Indians were different since they did not wash their fish before cooking it. Furthermore, they pointed out that the Lake St. John Indians were different since they did not turn the front end of their toboggan over as far as the Mistassini did and accordingly were considered to be sloppy workmen. Although there is no doubt that culture traits did vary within the area, data are insufficient to delineate local groups on this basis.

Lake Mistassini groups, however they might be distinguished, were certainly aware of one another. At the time of field-work, the Mistassini referred to or knew of a number of surrounding groups:

<i>piyAkokAmi inIw</i>	Lake St. John
<i>sekUtimi inIw</i>	Chicoutimi
<i>pesceyami inIw</i>	Bersimis
<i>opicIwAn inIw</i>	Obijuan
<i>cIsesipi inIw</i>	Fort George ?
<i>isImanIwsipi inIw</i>	Eastmain

<i>winlpekU</i>	Rupert House
no name recorded	Waswanipi
although known	
no name recorded	Kaniapiskau
although known	

The people of the Mistassini region had relationships with at least some of these groups. It has been mentioned that intermarriage occurred rather frequently over a wide area, generally resulting in the movement of females and only occasionally of males.

Trade relations were another form of contact. Contemporary accounts record that the Mistassini traded regularly with hunters from Nichicun, Neoskweskau, and Fort George (Burgesse 1945: 25). Pipestone was imported from James Bay by the Mistassini, with the Rupert House Indians acting as traders and middle-men. The manufactured pipes were traded as far away as with the Indians of the St. Lawrence River. Furthermore, the northern Mistassini obtained moose hides from the south by trade, and porcupine quills for embroidery from the Nichicun Indians. Finally, hare skins were also traded (Lips 1947: 443-4).

As early as 1634, the Montagnais, which may have included the Mistassini, were giving moose hides to the Huron for "cereals," "Indian corn," and "tobacco" (Thwaites 1896-1901, 6: 273). In 1661, Nekouba, or Lake Ducharme, situated approximately halfway between Lake St. John and Lake Mistassini, was said to have been "noted for a Market that is held there every year" (Thwaites 1896-1901, 46: 275). Undoubtedly, relations were maintained for a considerable period of time with neighbouring peoples within the area surrounding Lake Mistassini.

Although adjacent Indians appear to have maintained friendly relationships, there were periods of hostility. Although cases are few, feuds which occurred were settled by witchcraft. Such a case was recorded in the early 1600's in which one shaman used his religious power against a rival (Thwaites 1896-1901, 6: 195-9). In 1672, Albanel wrote that an old man of the Mistassini was at odds with a neighbouring "captain" of the "Mataoiriou nation" (Thwaites 1896-1901, 56: 165-9).

As can be imagined, group size is difficult to determine. By utilizing those population figures for the Round Lake Ojibwa and Mistassini Cree that always refer to the inhabitants of a territory of known size, estimates of population density have been determined (Table 3).

The problem of fission and fusion has been mentioned in the discussion of the Round Lake Ojibwa. In the case of the Indians living in the region of Lake Mistassini, these same processes undoubtedly occurred in the past. Unfortunately, this feature of their existence has been obscured by the establishment of trading posts which became foci for groups living within the vicinity. Since they spent summer after summer at one trading post, they began to fuse and to look upon themselves as an entity. When a post was closed, they disbanded and traded elsewhere forming new groupings. Previously, groups amalgamated because of a decrease in numbers, and with expansion they undoubtedly broke apart. Such changes were probably the result of alterations in game resources.

THE CREE-OJIBWA OF THE EASTERN SUBARCTIC

In the preceding sections of this paper, attention was given to two groups of Indians resident within the Eastern Subarctic, the Mistassini Cree and the Round Lake Ojibwa. Now it would be appropriate to examine data on the same topics from the entire area in a historical light.

The first feature to be studied is that of named groups. In the literature of the past three hundred years is found a multiplicity of names for Indian groups, not always representing comparable units, which circumstance tends to confuse the issue. Generally speaking, there are three main classes of names designating the Indians of the Eastern Subarctic. There are those broad designations covering a large number of Indians which, one suspects, are in many cases of European origin or, perhaps, of Indian origin. Secondly, there are those names applied to small groups of Indians which, in effect, may be local groups or several local groups inhabiting a particular drainage basin. Many of these names are probably of Indian origin, but there may be instances of European designations. Thirdly, there are indigenous clan names used only by the Indians in the southern part of the Eastern Subarctic. In addition to these classes of names, there is also the problem of cognates and the use of different names for the same people by different neighbours. Finally, to further complicate the problem, there was a tremendous movement of peoples in the southern part of the Eastern Subarctic who may have had new names given to them or may have retained their original names or those applied to them.

During the French régime, population shifts, especially along the Ottawa Valley and in the Great Lakes area, present difficulties. The French had several broad designations for a number of local groups such as Algonkians and Montagnais (Biggar 1929, III: 133). According to Dablon, the 'Outaouaks' were the first Indians to come down to trade, and therefore this name was applied to all groups of 'Upper Algonkians' (Thwaites 1896-1901, 54: 128-30). Furthermore, the different groups that gathered at Sault Ste. Marie were known as "Aoueatsionaenronnon," meaning those "who inhabit the coast of the sea" (Thwaites 1896-1901, 30: 113).

In addition to these broad groupings, the French also referred to a number of smaller units, which appear to have been local groups and might be conceived of as 'bands.' Champlain, in 1615, spoke of the "Otaguottouemins" who lived on the Ottawa River (Biggar 1929, III: 38), the "Nipissings" from the lake of the same name (Biggar 1929, III: 40), and at the mouth of the French River he met the *Cheveux relevés* or "High Hairs" (Biggar 1929, III: 43).

Along the north shore of the St. Lawrence River the Jesuits reported the "Chisedech" (Thwaites 1896-1901, 18: 227) and the "Papinachiouekhi" (Thwaites 1896-1901, 47: 221). The Indians of Tadoussac were in contact with a group referred to as "Porcupine," who, in turn, had contact with groups farther inland (Thwaites 1896-1901, 18: 227). Farther west were the "Attikameques" who lived inland from Three Rivers and were said to have contact with three to four other small groups farther north (Thwaites 1896-1901, 18: 227). As one proceeded up the Ottawa River, the first group was named the "Ouaouechkairini," or *petite nation* of the Algonkians, and

the second the "Kichesiprini" or "the savages of the Island." To the north of this latter group were the "Kotakoutouemi" and to the south were the "Kinouncheprini," the "Mataouchkarini," the "Ountchatarounounga," the "Sagahiganirini," the "Sagnitauouigama," and the Huron (Thwaites 1896-1901, 18: 229). In the vicinity of Lake Nipissing were the "Nipisiriniens" and to the north the "Timiscimi," the "Outimagamai," the "Ouachegami," the "Mitchitamou," the "Outurbi," and the "Kiristinon." The latter were said to live on the shores of the North Sea (James-Hudson bays) (Thwaites 1896-1901, 18: 229). On the east coast of Georgian Bay and north of the Huron territory were the "Ouasouarini," and farther north the "Outchougai" (Thwaites 1896-1901, 18: 229). At the mouth of the French River were the "Atchiligoum" and to the west the "Amikouai," followed by the "Oumisagai" and at Sault Ste Marie the "Baouichtigouiam." Beyond Sault Ste Marie lived the "Roquai" and to the north the "Mantoue" (Thwaites 1896-1901, 18: 231). In 1670, the "Pahouitingwach Irini," or *Saulteurs* as the French called them, lived at Sault Ste Marie. To the north were the "Outchibous" and the "Marameg," and to the east the "Achiligouiane," the "Amicoures," and the "Mississague" (Thwaites 1896-1901, 54: 127).

Raudot, in 1709, enumerated the following groups starting on the north shore of the St. Lawrence and proceeding westward: "Oumiamis," "Chicoutimiens," "Papinachois," "Montagnais," "Algonquins" of the St. John River, "Large and Small Mistassins," habitants of "Nemisco," "Aticameques," or "Poissons-blancs," "Monsonis," "Pisouogagamis," "Abitibis," "Machatamitibis," or *Têtes-de-Boules*, "Temiscaminques," and "Cristinaux" (Kinietz 1965: 365-6). In addition, he spoke of the "Amiquoues" who lived along the north shore of Lake Huron, the "Mississague" who lived on the river of the same name, and the *Saulteurs* who lived at the entrance to Lake Superior (Kinietz 1965: 370-1).

Dobbs (1744) spoke of several named groups, but in certain instances it is difficult to know whether these were local, named groups or a number of local groups. He mentioned the "Assinibois" and the "Cree" who lived toward Lake Winnipeg (1744: 20-1) and the *Sauteurs* who inhabited the region about Lake Winnipeg (1744: 21). On the east side of Lake Huron lived the "Ishisageck Roanu," and on the north side were the "Kirhawguagh Roanu" ("People of the Wilderness") and the "Missada" Indians (1744: 27, 28, 31). To the east of Michipicoten lived the "Epinette Nation" and to the west the "Ouassi," both of which Dobbs considered *Sauteurs* (1744: 32). On the west side of Rainy River lived the "Monsoin" or *Gens de Original* (1744: 33), on the north side of Lake of the Woods were found the "Monsoni" and "Sturgeon" Indians, and on the northeast side were the "Cris" (1744: 34). Between Lake Winnipeg and Lake of the Woods were the "Migechichilinous" or "Eagle-Eyed Indians" (1744: 35).

This plethora of named groups certainly suggests that during the first hundred years of contact no one group was very large because of the limited resources on which it could exist in this region. Perhaps, therefore, many of these names represent local, named groups or, in some cases, several local groups inhabiting a river drainage basin.

In contrast to the French, the British and Canadians at a later date frequently spoke of large named groups rather than local, named groups when

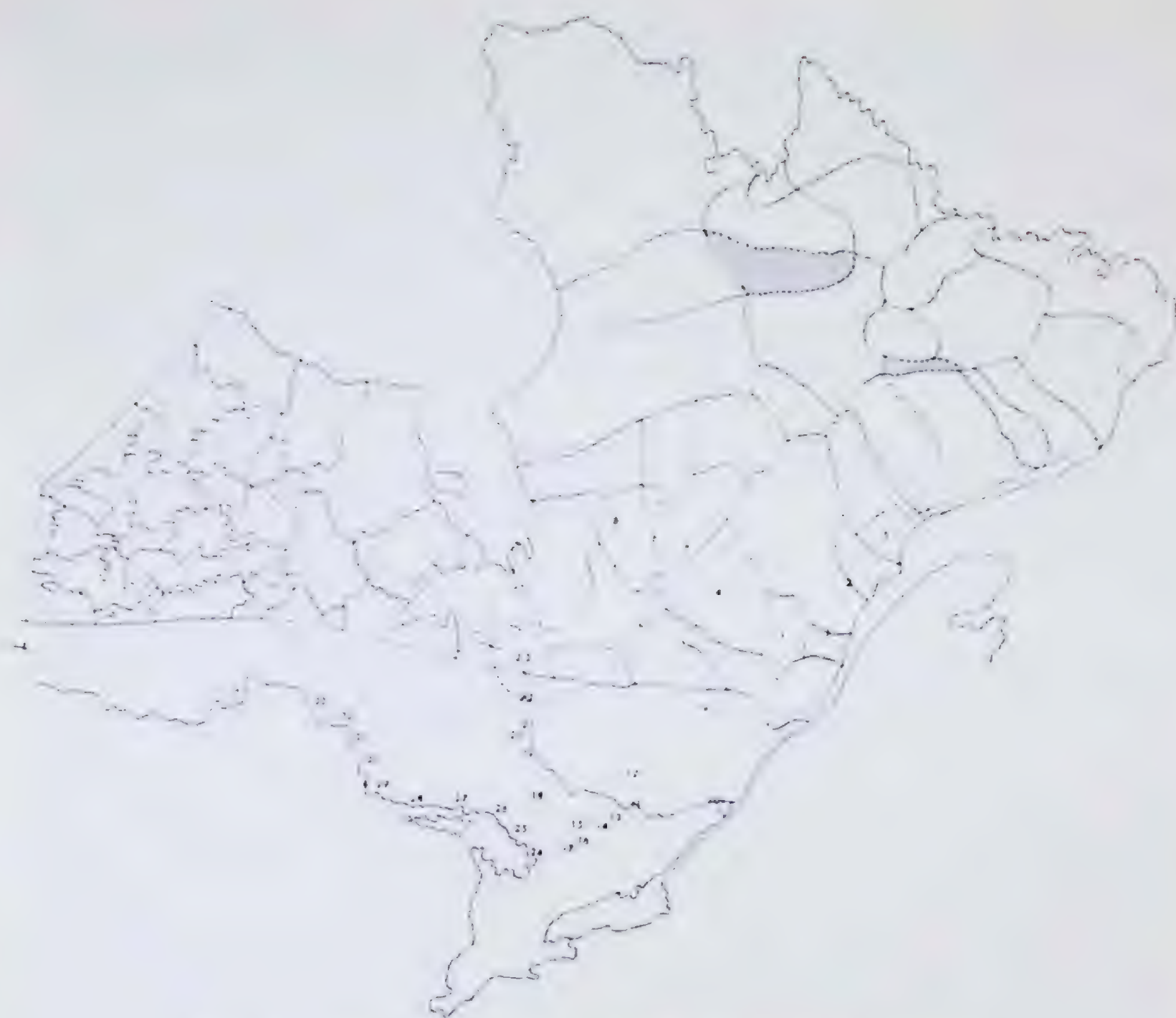
describing the Indians in the area of the Eastern Subarctic. Henry the Elder, in the late 1700's, did, however, speak of the "Missisakais" as inhabitants of the north shore of Lake Huron (Bain 1901: 35) and of the "Nipissing" (Bain 1901: 30-1). Interestingly enough, he mentions the "Maskegons" living at the mouth of the Demoinés River where it enters the Ottawa River (Bain 1901: 26). Franklin, however, spoke of "Maskegons" as inhabitants of the Winnipeg River area (Franklin 1824: 96n), while Hind (1860, I: 112) stated that the "Swampy Cree" or "Muskaigoes" lived northeast and north of Lake of the Woods. Ray spoke of the "Crees of the Wood," "Maskeg," or "Swampy Crees" as residents of Moose Factory (Ray 1882: 484), and Franklin placed "Swampy Cree" at York Factory (Franklin 1824: 38).

The term *Saulteaux* was applied to Indians in particular areas as well as to groups speaking a particular dialect. McLean, for instance, stated that the "Ottawa" from Macinac spoke *Saulteaux* (Wallace 1932: 25-6) and that the Indians of Fort Coulonge, called, by the French, *Têtes-de-Boules* and, by other Indians, "Men of the Woods," spoke *Saulteaux* (Wallace 1932: 40). Back located the *Saulteaux* in an area from Fort Alexander to Rainy River (Back 1836: 348). Franklin spoke of "Chippewa" at Lake of the Woods (Franklin 1824: 96n). Hind placed "Ojibwa" east and southeast of Lake of the Woods (Hind 1860, I: 112), while Richardson placed them on the north side of Lake Huron, along the entire shore of Lake Superior, and from there to Lake Winnipeg (Richardson 1851, I: 70-1). He also stated that the "In-ninyu-wk" inhabited an area from Sault Ste Marie to the Saskatchewan and Churchill rivers (Richardson 1851, I: 70). Graham wrote that the "Nakawawche" lived some one hundred miles inland from the coast of Hudson Bay (Rich 1949: 314).

Some smaller groups were also named at this time. McLean mentioned the "Mississagays" as neighbours of the "Algonquians" and said they spoke the same language (Wallace 1932: 109-10), Ray spoke of the "Abitibi" and "Temmiskaming" Indians (Ray 1882: 488), and Keating listed ten named groups from Lake Superior west to the vicinity of Lake of the Woods (Keating 1959, II: 149-50). He stated that the "Chippewas" were divided into small bands of a few families each with local names (Keating 1959, II: 149).

The above data suggest that at the time of contact and for a number of years thereafter, there were in existence in the southern part of the Eastern Subarctic relatively small, localized groups either named by the Europeans or having names for themselves (Map 2). The latter would generally seem to be the case. With the intensification of the fur trade, especially under the British, these local groups became dislocated, mostly westward, in their search for furs; intergroup conflicts increased, and groups became intermixed. In time they lost their identity and became subsumed under larger group names. The names for these larger groups sometimes seem to have been derived from one of the former small local groups such as *Saulteurs* (*Saulteaux*) or 'Outchibons' (*Ojibwa*). There was no large group known as *Ojibwa* or *Saulteaux* at the time of the French. Furthermore, the establishment of trading posts in place of mission stations perhaps encouraged the formation of larger groups of diverse origins from the surrounding areas.

The yearly cycle of these local, named groups seems quite similar to that described for the Round Lake *Ojibwa* and Mistassini Cree. In the summer



MAP 2 — *Early Local Groups Within the Eastern Subarctic.*

Early Local Groups within the Eastern Subarctic

- | | |
|---|--|
| (1) "Chicoutimiens" | (18) "Nipisiriniens," "Nipissings" |
| (2) "Papinachois" or "Papinachi-
ouekhi" | (19) "Timiscimi," "Temiscamingues,"
"Temiskaming" Indians |
| (3) "Chisedech" | (20) "Outimagami" |
| (4) "Algonquins of the St. John River"
or "Porcupine" | (21) "Ouachegami" |
| (5) "Oumiamis" | (22) "Mitchitamou" |
| (6) "Large Mistassins" | (23) "Abitibis," "Abitibi," or "Outurbi" |
| (7) "Small Mistassins" | (24) "Ouasouarini" |
| (8) "Nemisco" | (25) "Outchougai" |
| (9) "Aticameques," "Attikameques," or
"Poissons-blancs" | (26) "Atchiligoum," or "Cheveux
relevés"? |
| (10) "Ouaouechkairini" or "Petit
nation" of the Algonkians | (27) "Amikouai," "Amiquoues" |
| (11) "Kichesiprini" or "the savages of
the Island" | (28) "Oumisagai," "Mississague,"
"Missada," "Missisakais" |
| (12) "Kotakoutouemi" | (29) "Baouichtigouiam" or "Pahouiting-
wach Irini"? |
| (13) "Kinouncheprini" | (30) "Roquai" |
| (14) "Mataouchkarini" | (31) "Mantoue" |
| (15) "Ountchatarounounga" | (32) "Outchibous" |
| (16) "Sagahiganirini" | (33) "Marameg" |
| (17) "Sagnitaouigama" | (34) "Machatamitibis" or
"Têtes-de-Boules" |

a favourable fishing locality was selected where all the members, or most of them, would gather; then they would scatter into the forest with the approach of winter to seek a living and then unite the following spring. Dablon, for instance, in 1670 stated that several groups in the vicinity of Sault Ste Marie were in the habit of gathering there during the summer to fish (Thwaites 1896-1901, 54: 131, 133). It was recorded that in 1645-6 the Nipissings dispersed through the woods for the winter (Thwaites 1896-1901: 30, 125). Oldmixon's data suggest a similar pattern for the yearly cycle among the Indians of James Bay (Oldmixon 1741, I: 548). A similar situation existed among the Indians inhabiting the north shore of Lake Huron (Blair 1911, I: 279-80).

Group composition can only be discussed briefly since the available information is very limited. It would appear that each group had a leader generally with little power or authority. According to Oldmixon, each group of Indians in particular "districts" had an *Okimah* who was an elder looked upon for his experience and wisdom.

He has no authority but what they think fit to give him upon certain occasions. He is their speech-maker to the English: as also in their own grave debates when they meet every spring and fall. . . . (Oldmixon 1741 I: 548)

A similar situation (cited earlier) has been reported for the area inhabited by the Mistassini. Leadership was, however, more strongly developed among those groups of Montagnais-Naskapi, dependent upon the Barren Ground caribou, in the northern part of the Labrador Peninsula (Tanner 1944: 685; Rogers 1965). To hunt the caribou most effectively, a strong organization under the direction of a chief given a semblance of power and authority may have resulted. Elsewhere within the Eastern Subarctic this was not the rule, although those groups along the southern margins of the Eastern Subarctic may have been more structured. However, McLean reported that the chiefs in the Ottawa Valley in the 1820's had relatively little power and authority (Wallace 1932: 110). It would seem, therefore, that the leader was simply *primus inter pares*.

Presumably, the leader was a powerful shaman, but no doubt there were several in each group. This situation arose because generally a member of each hunting group, usually the leader, was a shaman.

Besides the above features of group structure, there is the following report by Erlandson in 1833 pertaining to the Indians near South River Post, 120 miles inland from Fort Chimo. He stated:

These, mostly young men are retainers of or rather drudges to the would-be great men: their employment is to attend nets and draw sledges. If any of them be industrious enough to set a trap and catch a marten, it is claimed by his master. In this state of bondage he remains 'till he can persuade a girl to bless him with her company; he is then a man. (Davies 1963: 222-3)

Mistassini informants reported the same custom; a young man was attached to an elder male rather than to his own father. How widespread this custom was among the Indians of the Eastern Subarctic is not known. It may, in effect, have been a form of bride service, although this is not made explicit.

Data are extremely limited and vague regarding the existence of boundaries for the territories of the local, named groups referred to previously. Oldmixon did say,

The Indians of certain districts, which are bounded by such and such rivers . . . and he goes on,

. . . as also in their own grave debates, when they meet every spring and fall, to settle the disposition of their quarters for hunting, fowling, and fishing. Every family have their boundaries adjusted, which they seldom quit, unless they have not success there in their hunting, and then they join in with some family who have succeeded. (Oldmixon 1741, I: 548)

This certainly suggests some form of boundary to the group territory within which the hunting groups held particular hunting areas. These were adjusted annually according to existing population and game densities and distribution. Albanel, in 1672, was stopped by the Mistassini from entering their area until permission was given (Thwaites 1896-1901, 56: 173); this suggests some concept of territorial rights on their part. However, the literature provides no further clues to exactly what these territorial rights might have been, nor how the people themselves conceived of the land.

Although there are no data regarding cultural homogeneity within any of these local, named groups, there is information of a limited nature pertaining to group relationships. As has been specified previously, there was a certain amount of inter-group trade, possibly more extensive than has been previously realized. Furthermore, among the groups along the southern margin of the Eastern Subarctic, hostile actions, undoubtedly in some cases stimulated by the fur trade and the search for pelts, were to a certain extent undertaken against neighbours. The "Bersiamites," in 1645-6, were said to be at war with Indians on the south shore of the St. Lawrence River (Thwaites 1896-1901, 30: 139). At a somewhat earlier date it is stated that the "Amikoues" were attacked by the "Nipissings" (Thwaites 1896-1901, 10: 83). Anderson, in the 1850's, reported a fight between the "Suckers" and the "Sturgeon" people in the Upper Albany River area (Anderson 1873: 80). Presumably, in spite of the hostilities, intermarriage between groups took place, both in the southern and northern parts of the Eastern Subarctic. Of course if Hickerson is correct that these local, named groups were "local exogamous kindreds," then intermarriage was mandatory (Hickerson 1962: 76-8). The evidence, however, does not yet seem to be sufficient to substantiate this opinion.

Limited amount of data concerning group size during the past three hundred years is summarized in Table 4. Although group size appears to decrease as one moves northward within the Eastern Subarctic, the figures given in Table 4 must be used with caution. Many of them represent the total number of people who traded at a particular post; they therefore include more than one local group and are, accordingly, too large. Furthermore, in some instances the figures are perhaps less than they typically should be as a result of the temporary reduction of population through disease and (or) starvation. Nevertheless, they do give some indication of group size.

It would appear that in the northern part of the Eastern Subarctic group, size ranged from approximately fifty to perhaps as many as a hundred or a

hundred and fifty individuals. This latter size may have been the result of the coalescence of two or more local groups. In the south, group size appears to have been slightly higher, varying from perhaps a hundred people to several hundred although the Nipissing and *Cheveux relevés* appear to be exceptions. Again, it may be that several local, named groups were combined and that the terms were inclusive.

CONCLUSIONS

It seems reasonable to make some general statements on the basis of the data provided in the preceding sections. One must keep in mind, however, that there are three rather distinct types of what have been called, in the past, 'bands.' First, there is, of course, the aboriginal local group; secondly, the trading post 'band,' and finally, the government 'band.' The primary concern here is the aboriginal local group, but since data are meagre for this early period, it is necessary to draw from reports describing the other two types and to assume that, in certain cases, the tradition was carried on from the earlier aboriginal 'band.'

There is no doubt that local, named groups existed at the time of contact and, in some cases, maintained themselves within the remoter areas of the Eastern Subarctic, until a very recent date. Furthermore, since these groups subsisted by hunting and fishing, they remained dispersed in hunting groups during the winter months coming together at a favourable fishing site in the summer. Each group appears to have been composed of closely related families under the leadership of one of the elder males who also possessed much religious power. However, political authority was weakly developed. Finally, within each group there were a number of individuals who were recognized as shamans, probably one for each hunting group.

TABLE 4

Year	Group size	Number of people
1615	On the Ottawa River a named group spoken of as containing a few Algonquins (Biggar 1929, III: 38) Nipissing — 700 or 800 <i>Cheveux relevés</i> — 300 men (Biggar 1929, III: 40, 43)	700-800 1,500
1640	Chisedech and Bersiamite, <i>insignificant</i> nations: Attikameques trade with 3-4 other <i>small</i> nations to the north (Thwaites 1896-1901, 18: 227)	
1670	<i>Saulteurs</i> — 150 Achiligoniane — Amicoures — Mississague — 400+ Two Nations north of Sault Ste. Marie — 500 (Thwaites 1896-1901, 54: 133)	150 125-150 250
1820	North of Kaniapiskau — 100+ together in June (Davis 1963: 49) Great Whale River — nearly 100 in Aug. (Davis 1963: 89)	100 100
1842 (approx.)	Naskapi — 100± men (Wallace 1932: 258)	500

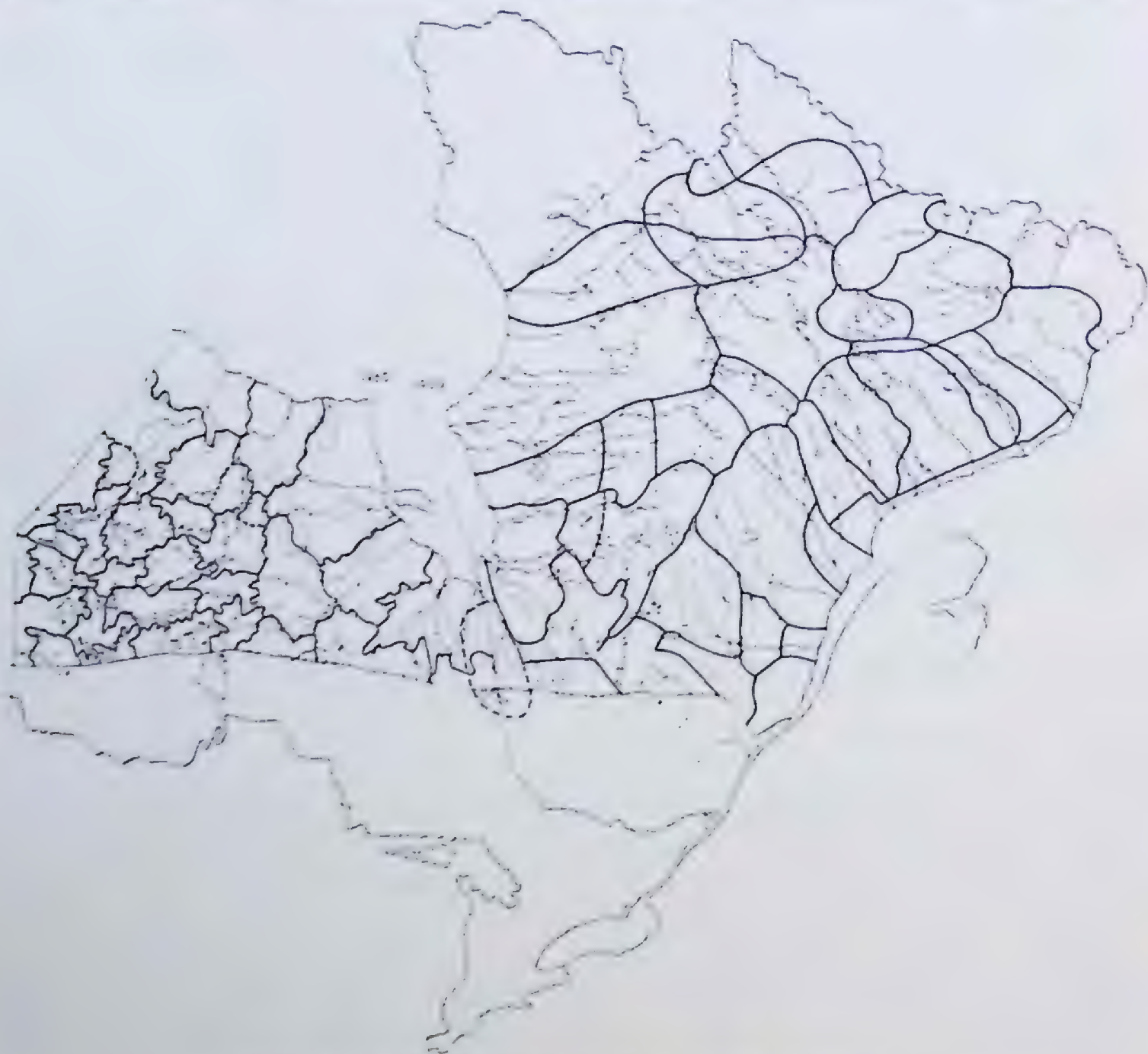
TABLE 4 (Conc.)

Year	Group size	Number of people
1852	Osnaburgh House — 50-60 families	250-350
	Lac Seul — as many as 80 taking debt (Anderson 1873: 69)	400
	Albany — July — 50± (Anderson 1873: 100)	50
	Aug. — 45± families (Anderson 1873: 198)	225
	Long Lac — 60± (Anderson 1873: 194)	60
1855 (approx.)	Berens River Post — 50-60 families (Ryerson 1855: 80)	250-300
1868	Brunswick House — 150 (Batty 1893: 61)	150
	Rupert House? — 300-400 (Batty 1893: 62)	300-400
1868	Northwest River — 154 (McGee 1961: 31)	154
1885	Mistassini — 26 families (Low 1885: 17D)	130
1886	Trout Lake — 500 (Miller 1912: 100)	500
1892-93	Fort Chimo — famine 350-200 (Low 1897: 41L) 250-150 (Low 1897: 122L-123L)	350-150
1895	Waswanipi — Mistassini — Nichican — 300 (Low 1897: 41L)	100
	Mistassini — 25± families (Low 1897: 70L)	125
	Nichican — 13± families (Low 1897: 101L)	65
	Great Whale — Fort George — Rupert House — 1,000 (Low 1897: 41L)	330
	Northwest River — Davis Inlet — 200± (Low 1897: 41L)	100
1903	Winisk River — Attawapiskat River — 700± (Miller 1912: 131)	350
1913	St. Augustine — 25 families (Bryant 1913: 5)	125
1913-18	Mistassini — 250± (Anderson 1961: 91)	250
1914	Voicey Bay — 250 (Waugh 1925: 128)	250
	George River band — 70-80 (Waugh 1925: 128)	70-80
	Fort Chimo — 200-300 (Waugh 1925: 128)	200-300
1928	Voicey Bay — 56	56
1939	Voicey Bay — 100 (Tanner 1944: 677)	100
1950	Great Whale River — 171 (Honigmann 1962: 4)	171
1952	Northwest River — 190 (McGee 1961: 32)	190

Boundaries and the concept of territoriality would appear to be another important feature of these local, named groups. From existing evidence the territories coincided with parts or all of river basins (Map 3). This, it is

postulated, is related to canoe travel in the spring and fall, and to winter dispersals and summer aggregations. Although it was possible to move much more freely with a toboggan, canoe travel was easier and weights were less limited. However, is this feature pre-contact or the result of the establishment of trading posts at the mouths of rivers, influencing the Indians in their mode of travel?

The multiplicity of terms that the Mistassini Cree had for various water features suggests that the local group was confined to a river basin and its activities were oriented towards the water, even though the surfaces of the lakes and rivers were frozen for the greater part of the year. These terms are as follows: "lake" (*sakAhikAn*), "round lake" (*wowIyekAmaw*), "long, narrow lake" (*sakokAmaw*), "lake with cliffs along the shore" (*ciscekakAmaw*), "small lake near a large lake" (*nAtAscesatIstowAw*), "last lake at the head of a stream" (*macIsacIstowAw*), "lake with no inlets or outlets" (*pIkUtawUh-UhkU*), "point of land extending into lake" (*mAcIsteweyaw*), "bay in lake" (*wasIhaw*), "straight shore of a lake" (*kweskwAweyaw*), "narrows in a lake" (*opaw*), "narrow in a lake with rapids" (*opacewUsIw*), "shore" (*yayew*), "on the far shore" (*AkanIhc*), "outlet of a lake" (*AkUhItAn*), "inlet of a lake" (*wiyahkokAmaw*), "a constricted bay" — river or lake? (*yatokAmi*), "island" (*mInIstIku*), "rocks extending slightly above the surface of the water in shallow areas, occasionally supporting small shrubs but not trees"



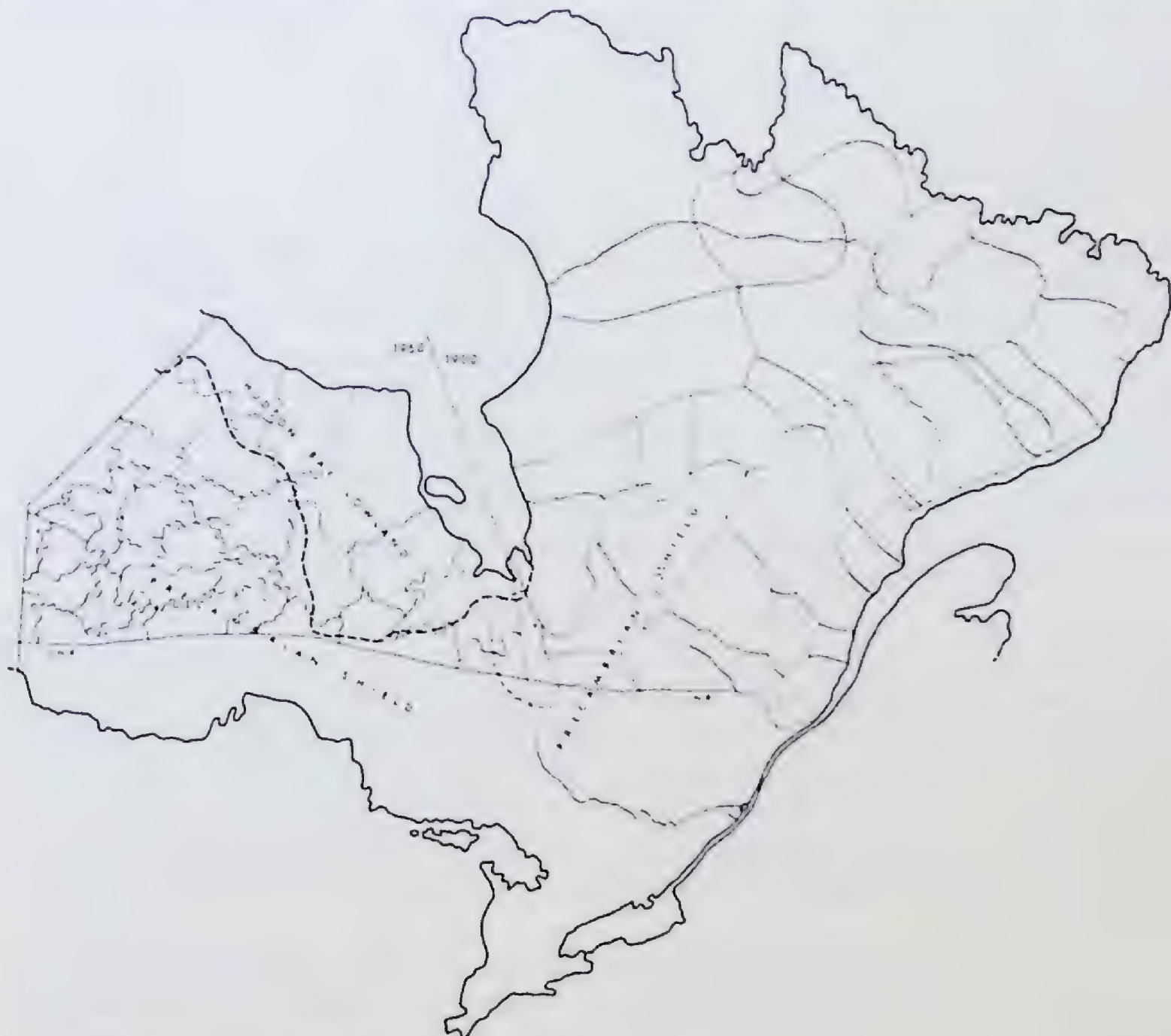
MAP 3 — Bands in relation to Drainage Systems

(*nInIstIkopIskU*), "muskeg" (*nAsckU*), "stream or river" (*sipi*), "small stream or brook" (*sipisIs*), "rapids" (*postIko*), "the widening of a stream" (*natoyaw*), "bay in a stream" (*wasAhaw*), and "at the foot of the rapids" (*seskacIwAn*).

Streams were named after the lake from which they discharged. They retained this name until they joined another large stream or entered a lake. Below this point a stream was known by the name of the stream or the lake it joined, retaining the name until the river joined another stream or lake. The naming process was then repeated.

Names were given to particular locations, sometimes several square miles in extent, where a number of streams joined or entered a lake. On occasion the area between two neighbouring lakes connected by a short discharge was named. These areas, well known to the Mistassini, were often used for camp-sites.

The decrease in the size of territories to the west appears to be correlated with increased game densities and with the fairly constant local group size throughout this entire area. Territorial size is quite large on the Hudson Bay Lowland, but this area, like the Labrador Peninsula, is extremely poor in resources (Map 4). It must be remembered that territorial size on the Precambrian Shield of Western Ontario could be more extensive if group



MAP 4 — *Band Territorial Size:
The Hudson Bay Lowland versus the Precambrian Shield.*

size was larger. In this case it is not the environment that limits territorial extent but rather other features limiting the size of the group.

There is no evidence available to indicate whether these small local groups had any cultural homogeneity distinguishing them from neighbouring groups. It is suspected that they did not. Rather, there were a number of these groups sharing certain minor cultural items or peculiarities to the exclusion of other groups.

In general, group relations seem to have been of a friendly nature. There was intergroup trade and marriage. Occasionally, in the southern part of the Eastern Subarctic, hostilities occurred between neighbouring groups.

Throughout most of the Eastern Subarctic, with the possible exception of the southern boundary, group size remained fairly constant, approximately one hundred individuals per local group. This might drop to as low as fifty to seventy-five persons or it might go as high as one hundred and twenty-five to one hundred and fifty. The larger groupings that have been recorded apparently were the result of the establishment of trading posts where several local, named groups gathered.

Although group size appears to have remained fairly constant throughout the entire area, population density was much less in the east than in the west. The following figures exist for the east: Great Whale River, one hundred square miles per person (Honigmann 1962: 5); Mistassini, in 1953-4, sixty-six square miles per person (although they need at least one hundred square miles per person to be self-sufficient and it is estimated that in 1829 the density was two hundred square miles per person (Rogers 1963: 22)); Waswanipi, ninety-three square miles per person ((Davidson 1928: 46, 47, 57-9) based on data there, and the assumption that there are approximately three persons per married male); Grand Lake Victoria, fifty-five square miles per person (Davidson 1926: 91, 93, 95). On the Hudson Bay Lowland the population was sparse: Attawapiskat, thirty-two square miles per person (Honigmann 1962a: 19); and Winisk, one hundred square miles per person (Liebow and Trudeau 1962: 191). On the Precambrian Shield of Western Ontario, densities were somewhat higher: Round Lake Ojibwa, twenty-two square miles per person (Rogers 1962: A2, B8); Pekangikum, twelve square miles per person (Dunning 1959: 49); and Berens River, six square miles per person (Hallowell 1949: 40). Unfortunately, nothing can be said regarding fission and fusion among these local, named groups.

Perhaps one can define the local, named group or 'band' as a loosely structured unit with a patrilineal bias, comprising seventy-five to a hundred and twenty-five people, inhabiting a drainage basin alone or in conjunction with other such groups, uniting during the summer on the shores of a lake within the territory and dispersing for the winter in groups to hunting areas. These groups were frequently bilateral extended families although ideally patrilineal extended, all of whom were generally under the leadership of a head man whose position was *primus inter pares* and who acted as the leading religious expert.

What are the factors permitting or promoting the establishment of such a social unit as this type of band among the Indians of the Eastern Subarctic? It appears that they involve in part an adaptation to a severe environment when exploitation is at the technological level of hunting and fishing. Within

the Labrador Peninsula, group size appears to have been restricted since transportation facilities were not adequate to allow a larger group to exploit the necessarily extensive territory needed to supply it with sufficient food. Farther west, the small size of the local group may be the result of fission caused by feuding within the context of shamanism. On the other hand, any group was held together by the shaman's abilities, and these abilities, in turn, had some of the role attributes of the group leader. Nevertheless, leadership was weakly developed since the maximal group was dispersed for the greater part of the year. This, in effect, meant there was little reason for having a leader. Furthermore, there was a concept of individualism that hindered the development of authoritarian leaders except in a charismatic manner and the belief or philosophy allowing individuals to escape domination; a belief that no one had the right to deny a man movement to other areas in search of game. Kinship and a common cognitive orientation were other factors uniting the group. Finally, the necessity to find spouses was another reason for assembling in a larger group or band during the summer months.

NOTES

1. I wish to thank the National Museum of Canada, and especially Dr. David Damas, for inviting me to give a version of this paper at a conference held by them in Ottawa, August 30–September 2, 1965. I wish to thank Drs. David Damas, Thomas B. Hinton, James VanStone, Mr. Camil Guy, and my wife for their critical reading of a draft of this paper. All errors of judgment are the sole responsibility of the author.
2. I am grateful to the Ontario Department of Lands and Forests for permission to reproduce their map of band areas in Northern Ontario. I have modified the map slightly on the basis of field-work in the area. Band distribution represents for this area the approximate situation as it has existed for the past thirty years. The distribution of bands within the Labrador Peninsula is based upon Speck (1931) with minor modifications in the southwestern portion based upon my own field experiences. These are indicated by broken lines. There have, of course, been changes over the years, but this distribution of bands represents the situation as it existed twenty to thirty years ago. In the intervening region, band distributions have been compiled on the basis of Davidson (1926 and 1928) and Jenkins (1939: 28, Figure 28). The Canadian National Railway line was used as the southern boundary of mapped bands since there are almost no data to the south of this line. Inadvertently, the Lake St. John band became divided. Territories 20 and 24 should be considered as one. Those areas filled with diagonal lines were apparently not inhabited.
3. The term 'Weagamau' in Ojibwa simply means 'round lake.'
4. The term 'Round Lake Ojibwa' is used in this paper specifically for those Indians who gather at the village on the shores of Weagamow Lake and comprehensively for all the Indians of this area in the past, even though there were several local and distinctive named groups residing there.
5. There exists a problem as to what residence really means since it can be applied to two different types of groups, either the hunting group or the local group. In this paper no concern is given to residence patterns within the hunting group. Rather, it is residence within the local group that is considered.
6. June Helm (pers. comm.) has suggested that where men moved out they were often forgotten and this probably was the reason for the low figures given in the tables. But this should be equally or more true for the women, and yet a greater number of them were remembered.

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DISCUSSION

DAMAS: Is there an imbalance in the sex ratio in your area? I am asking because in the Central Eskimo area feuds were common and these have been attributed to rivalry over women. Could the feuds that you report be due to a similar population problem?

ROGERS: The feuds are not the result of rivalry over women in the sense of wife stealing, etc., but rather they were cases of fathers-in-law trying to bring sons-in-law into their groups. The rivalry grew up between men trying to build up their groups, especially in cases where females were preponderant in families and men were sought to help balance the ratio.

LEACOCK: We are comparing contrariwise some of the same material. That is to say, Speck reports for the Mistassini that, of seventeen marriages, six were definitely patrilocal, six definitely matrilocal, and one possibly patrilocal. The chief point we all note is that both forms of residence were present.

ROGERS: There are two problems involved here. (1) There is the necessity for matrilocal residence in the first three years of marriage, and it is a question of how many of these residence situations actually became permanent. (2) If a man had daughters, matrilocal residence would prevail because the old man could not carry on alone. He would not lose all the women out of the family. The Indians spoke of an ideal situation of a man with three co-resident married sons. Usually it was an ideal that could not be reached.

GARDNER: I wonder if it might not be profitable to take two approaches to residence rule with the Algonkian material. (1) Set up a list of priorities. As you say, the ideal situation was to have sons whose wives would come into the group. Therefore the first preference is for patrilocality. Given an inability to achieve this mode of residence we could ask what alternative choices there might be. (2) The other approach would be to make a statistical analysis of actual residence situations. This latter approach might reveal something approximating bilocality. Such a generalization would be important from the point of view of the composition of groups.

EGGAN: A time factor would have to be added to the latter approach because there are situations when a man may reside matrilocally for the first few years and then patrilocally for a period, and still later he might shift to another group.

ROGERS: Is there not also to be considered the problem of residence in the band versus residence in the hunting group? Then there is another problem. Four brothers may trap together in their youth, but by the time they are 50 they will probably have established their own independent units. If there is land enough, they may take up their own territories as they split apart.

EGGAN: It is very difficult for patrilocal residence to function in these areas of low population density and stable populations with semi-nomadic settlement. I think the argument you have advanced is an argument against

Speck's original formulation. Half the population would be left out in his scheme. The organization becomes more structured for the Round Lake Ojibwa who have a more favourable ecological situation and greater population density with a better resource base. It is interesting to note, however, that it is not structured patrilocally even though that may be an ideal. Rather, the family is organized bilaterally. The bilateral extended family is predatory; if you wish to build up your own group, you must steal people from others.

ROGERS: There are two points that I wish to stress. First, there seems to be more structuring as one proceeds westward. This is more impressive than data suggest. It still, however, must be proved. Secondly, the groups do not increase in size as one goes westward even though the productivity of the land increases. Hunting groups are constant in size across the area as are band sizes. The band size tends to be about twenty-five to fifty persons.

McKENNAN: Perhaps we ought to know more about your fifth criterion of bands: fusion and fission. I would suspect that there was competition within the bands for leadership. You have suggested that leadership has important charismatic qualities, and I strongly suspect that throughout the Subarctic many leaders were shamans. I would also suspect that whenever the bands grew beyond a purely patrilineal band level, there was compositeness and that when more than one extended family was represented, factionalism would be inevitable. Someone would be jockeying to be the new chief. This rivalry together with pressure on resources could result in the splitting of the band.

DAMAS (to Rogers): You spoke about being unable to find any significant differences from band to band. Do you think that you can circumscribe a marriage universe as I have tried to do in the Central Arctic?

ROGERS: There is an example of this in the four-band unit to which I have referred.

DAMAS: Will 90 to 95 per cent of the marriages be contracted within that unit?

ROGERS: No. Probably not that many.

HELM: There is the problem of whether or not we can talk about tribes in this context in so far as tribes indicate a political organization. The concept of 'tribal' identity spreads beyond the band. I have suggested that the larger identifying group corresponds to the marriage universe. The edges of this universe may be hazy — there are always some members who marry outside into what would be identified as another tribe. That is, it is a unit within which the majority of marriages occur with a leakage at the fringes.

ROGERS: Did not the Paris study of the marriage unit indicate a number of about 1,000 persons?

With regard to the band of one hundred or more individuals which I found in the eastern Subarctic, it is not an independent marriage unit. A group of four or five such bands is required to form a marriage unit. The band is

similar to the smaller hunting group in that it cannot exist by itself if it has to adjust to a changing environment.

HELM: There is also the problem of finding suitable mates. A man cannot wait ten years until a three-year-old girl reaches puberty. When he is about twenty he goes beyond the band to look for somebody else. In some cases he could go beyond his principal marriage area.

ROGERS: That situation is related to cross-cousin marriage as well. When a boy is about nine years old, his parents try to arrange his marriage according to the cross-cousin principle. It is often difficult to find someone in that category at the right time.

EGGAN: How far north is the linguistic division between Ojibwa and Cree?

ROGERS: While there may be a steep gradient in a marginal area, there is no actual point of separation. The linguists draw a line which is based on very fine distinctions.

DAMAS: Would there be the same sort of intergrading with regard to the marriage universe?

ROGERS: Probably. The Indians do have comprehensive names for larger groups, which on occasion did intermarry.

McKENNAN: There are similar problems in the Alaskan area. The so-called 'tribal divisions' are really linguistic divisions, and the languages have no sharp delineations.

EGGAN: The Round Lake Ojibwa are a rather unusual group for Ojibwa in that they do not have a clan grid. The clan system appears to become stronger as one moves southward, though even for the Berens River group Dunning has shown that the clan is rather insignificant. The Round Lake group carries the process to its logical conclusion: it gets rid of the clan or never accepts it. The clan system is a means of identification, and it is flexible in that its members may be scattered over a wide area, but in any given locality people may belong to only one clan.

Regarding the whole problem of the Ojibwa clans, someone should review the literature of the area in terms of modern conceptions because the totemic and clan situations are quite unusual. The clans do not seem to interfere with the bilateral local groupings in Canada, but across the border, among the Ojibwa of the United States, the lineage begins to have some corporate quality. It not only controls marriage but it also controls ceremonial life.

SLOBODIN: There is a similar situation among the Kutchin where clans are found near the edge of the area but not in the centre.

McKENNAN: I would say that they were more important in the west than in the east.

EGGAN: The clue to the importance of the clan among the northern Ojibwa is stated by Dunning; clan identification is used when a stranger joins

a group. Within the group there does not appear to be the need for this identification by clan since everyone knows everyone else anyway.

McKENNAN: Both Leacock and Rogers have used the term 'hunting partner.' Were these partnerships formalized? Were they based on marriage? I think that these are important points to consider because one of Service's criteria for bands is the lack of sodalities. If a hunting partnership is formalized, there would appear to be the beginning of a sodality. This is an important consideration when viewed in an evolutionary perspective.

LEACOCK: It is questionable whether these partnerships were formalized. I have histories of over twenty partnerships, including teams of up to six or seven men, with constantly changing alliances over a period of four years.

HELM: With regard to these hunting partnerships of two to seven men, are they task groups of men alone, or are women and children taken along on hunting trips?

ROGERS: There are a number of problems involved in using the term 'hunting partners.' Take for instance the situation at Mistassini where the families join together consistently every year. The men of the group leave for caribou hunts in the winter and also seek the moose which yard up at that season. Such parties would be comprised of all the men of a particular hunting group. I do not know whether or not such groups should be called hunting partners. They do act as a unit. Men usually work in pairs on the trap-line or when hunting small game. In fishing, the man and wife form the team. At Round Lake the men cooperate in pairs as hunters and trappers. Today there is a new pattern emerging. The men leave their families in the village, and the children attend school. Some of the trappers begin to cover their trap-lines at the beginning of November and return in December to remain in the village until the muskrat season begins. During that season the entire family leaves the village.

HELM: I was trying to make a distinction. You had been talking about the hunting partnerships when I mainly thought of the trapping partnerships in which situations the women remain in camp and the men go out alone. When the women are taken along, it is no longer a trapping partnership but rather a small localized group on the move — a minor society.

McKENNAN: There would then seem to be a contrast with the Western Subarctic; that is, among the Kutchin and in the Yukon area generally there are formalized institutionalized partnerships or bond fellowships which could be considered as the forerunners of sodalities.

SLOBODIN: Among the eastern Kutchin, partnerships may entail lifelong relationships, and they are partnerships in that they are dualities. While there might be the germ of a sodality among the Peel River Kutchin, the relationship is called *cəluk* or 'partner,' and usually young people of the same sex choose a partner and they remain close or best friends for the remainder of their lives. Not everyone has such a partner and you do not always trap with that person.

(To Rogers): You gave instances of diffusion of various culture traits which you seemed to attribute as a consequence of peaceful relations. I think that we could probably find many cases in which diffusion occurred under unfriendly circumstances.

ROGERS: There did not seem to be warfare in this area, at least not the kind where goods were captured that could be integrated in the society. Taking that into consideration I had assumed the transmission of cultural features could have taken place only through marriage between groups. Hostilities seem to have been mainly associated with witchcraft.

LEADERSHIP AND PARTICIPATION IN A KUTCHIN TRAPPING PARTY¹

By RICHARD SLOBODIN

RÉSUMÉ

En février et mars 1947, j'accompagnai un groupe de trappeurs kutchins jusqu'à la partie supérieure du bassin hydrographique de la rivière Peel, dans le Territoire du Yukon; les Kutchins parlent le dialecte des Athapascques de la zone subarctique américaine. L'origine du groupe, ses déplacements, son activité et ses occupations, dénués de caractère économique, sont mentionnés. Les membres de la partie de chasse s'étaient répartis suivant un mode de groupement très semblable à celui de la famille kutchin élargie. Presque tous appartenaient à un groupe descendant de la même parenté et appartenant lui-même à un type qui passait pour très important dans l'organisation sociale des Kutchins.

Le caractère de la fonction de chef ainsi que le rang social se manifestaient par les activités économiques, par les plaisanteries échangées au cours de la partie de chasse et par les relations qui s'établissaient entre le conducteur principal et le groupe allié par le sang. La théorie proposée par la sociologie américaine au sujet des petits groupes ne semble pas concorder avec la dynamique de la société kutchin.

In February and March, 1947, I had the opportunity of travelling with a trapping party of Kutchin Indians in the upper drainage of the Peel River, a watercourse which rises in the Cordilleras of northern Yukon Territory, Canada, and flows northeast to join the Mackenzie River near its delta on the Arctic Coast in the Northwest Territories. A brief account of this trip has been published earlier (Slobodin 1962: 47-53).

A description of the trip, of the party members, and of the party's structure as a primary group, with some discussion of the place of this group in the society of a hunting people in modern times is presented here. Consideration is given to historical and ideological factors and to interpersonal relations not obviously related to social structure, as well as to ecological and structural factors in the initiation of the group, in determination of its membership and activities, and, more particularly, in defining the roles of leaders and other group members.

THE PEEL RIVER PEOPLE AND THEIR HABITAT

The Kutchin Indians are an Athapaskan-speaking people of the western Canadian and eastern Alaskan Subarctic. The tribe at present comprises eight distinct bands, each occupying and named for a river drainage or a section thereof. Although much of their habitat is within the Arctic Circle — they are probably the most northerly of all American Indians — it is not within the true Arctic life zone, but rather in the boreal forest. The timberline, which in this area reaches a higher latitude than elsewhere in the western hemisphere, forms the boundary between Kutchin and Eskimo country. Spruce is the overwhelmingly predominant species of tree in this forest zone.

Land and water life is fairly abundant within the habitat of the Kutchin. Throughout their known history, the people have subsisted by means of hunting, trapping, and freshwater fishing, with plant-gathering an incidental activity. By far the most important mammal economically is the caribou. Muskrat, beaver, moose, snowshoe rabbit, and occasionally mountain sheep, black bear, and porcupine are hunted for food and in some cases for fur or skins. In addition to muskrat, the animals taken primarily for fur are beaver, mink, marten, lynx, wolf, wolverine, rabbit, ermine weasel, otter, fisher, and several colour variations of fox.

Within the Peel River drainage and the contiguous areas frequented by members of the Peel River band, the most significant regional distinction is between the mountain and plateau country of the upper Peel and the low-lying Mackenzie River plain. At the point where the Peel breaks out of the mountains onto the plain, some eighty miles above the confluence of the Peel and Mackenzie, "the transition . . . is very sharp" (Camsell 1906: 35).

The mountain region constitutes part of the Cordilleras system where it grades down toward the Arctic coastal plain. The sectors of this system within the upper Peel River drainage are the western Richardson and the Ogilvie mountains. These highlands are not lofty; the highest peak is probably no more than 6,500 feet in elevation. However, much of the highland terrain lies well above the altitude where trees can survive at this latitude, so that it is tundra and barren or glaciated mountain.

The plain is marked increasingly, as one nears the Mackenzie Delta, by a complex of lakes, swampland, and meandering streams characteristic of poorly drained glacial terrain.

INVITATION TO THE HUNT

In early 1947 I was fairly well acquainted with the Peel River Kutchin, a community then numbering about 375 persons, having sojourned with the band from September 1938 to May 1939. The principal advantage which accrued from the earlier visit was that when I returned to the Peel in 1946,² I was considered to be, not a formidable official sort of person, but rather, the fellow whom, as Chief Julius remarked on a festive occasion, the people had "taken in when he was just a poor boy."

It was, therefore, in an act of almost pure kindness, not untinged with condescension, that Willie Lake,³ a "second chief" or councillor of the Peel River band, invited me to join a trapping party under his leadership. This party proposed to search for marten in the Richardson Mountains to the southwest of Fort McPherson.

By the second week of January, a number of families had already left the settlement for family trap-lines or were at the point of departing in groups for the mountains. I looked toward the Lake group's departure with mixed feelings; while it was to be hoped that the trip would bring benefits to the party members, it would take several of my best informants away.

In the crowded Hudson's Bay Company store one morning, Willie Lake was lolling at the inner or darker end of the long counter. Catching my eye, Willie beckoned me with an almost imperceptible motion of the head. When I joined him, he said, "You know Stephen and I are taking a bunch upriver for marten. Want to come along?"

Willie Lake's bunch did not set out until several weeks later, as several members had business in Aklavik, while others had to visit a 'meat camp' west of the Mackenzie Delta to obtain caribou meat for members of their households who were remaining behind, mainly because of illness.

During this interval I accompanied another Indian on a short trip to his trap-line up the Peel. While we were having tea at a camp-fire some thirty miles from the settlement, my companion remarked, "Right now you're farther upriver than a lot of those people around the settlement have ever been. When you go into the mountains that will be good for you. You'll find out how the Indians really live. You'll see."

After a pause, he added, "Hunting muskrats is all right. I kill lots of rats. How can we live if we don't hunt rats? But that's not the real Indian way. That's good for 'breeds and the 'klavik bunch."

"But what about . . . ?" I named several white and Métis trappers who were known for travelling far into the mountains after fine fur.

"Sure, they are good hunters and tough travellers. Good men. But they don't hunt the Indian way. You go with Willie and Stephen, you'll see."

PEEL RIVER KUTCHIN GROUPINGS

Much of the career of any Peel River Kutchin is spent in the nuclear family, existing as a distinct physical and social entity, yet much time is also spent in other groupings. Although the nuclear family never loses its identity, upon occasion it joins with one or more other families to form a larger grouping. The modalities of these units in terms of size, in distinctiveness in function, and, for the most part, in structure, as well as discernible regularities in the occasions for their formation, make it possible to recognize certain extrafamilial groups as characteristic of Peel River Kutchin social organization.

1. *The paired family.* From time to time two nuclear families may combine. In a great majority of cases these are the families of siblings (including cousins) of the same or opposite sex with their spouses, minor children, and other dependants. The period of partnership may vary greatly in length but seldom exceeds a season — e.g., of winter trapping, spring muskrat hunting, or summer fishing — the families form a single unit marked by commensality, sharing of proceeds or profits from economic activity, and cooperative care for children and other dependants. The family heads are of the same age-group and, in most cases, function as joint heads of the paired family unit; if an individual seeks to dominate, the other is likely to withdraw from partnership, taking his family.

The relationship differs markedly from that obtaining in other types of households composed of two nuclear families, such as a newly married couple residing with the wife's parents, or a middle-aged couple with the aged parents of either spouse. The Kutchin image of these units is that of a single family with its dependants or semidependants.

2. *The trapping party.* Upon occasion, larger groups are formed for the purpose of trapping fine fur in the mountains. During the past century this type of grouping has ranged in size from four to eight families. In modern times men sometimes join the party without their families. It is said

that this kind of participation was rare in pre-contact and early post-contact times, when the band's activities were centred in the mountain area. During the nineteenth century, a trapping party was likely to remain intact throughout a winter; in recent times the average duration has been two months.

The trapping party forms around a leader and, in all known cases, a subsidiary leader, termed here the 'lieutenant.'

3. *The meat camp.* This is a wintertime aggregation ranging in size from 15 to 50 families whose movements are, for various reasons,⁴ limited to the neighbourhood of the largest accessible caribou population. In a group of this size, little trapping of fine fur is possible. During the past half-century, meat camps have been situated, as a rule, within one or two days' winter travel from Fort McPherson, Arctic Red River, and Aklavik. There is much coming and going between meat camps and settlements, and much visiting by whites and Métis.

4. *The fish camp.* After the breakup of ice in late spring or, in recent decades, at the close of muskrat hunting in June, families gather at favourable riverine fishing sites, remaining there throughout much of the summer. The range in size of the fish camp (10 to 30 families) appears to be slightly lower than that of the meat camps. Therefore, there are likely to be two or three fish camps coexistent during a summer, whereas if a meat camp is formed during a winter, there will rarely be more than one. Certain favourable locations repeatedly have been, or were formerly, the sites of fish camps; some of these are designated "Indian Village" on Canada Hydrographic Maps.

Meat camp and fish camp share certain structural features in addition to a constituency of nuclear families. These are matrilineal sib, wealth-ranking, and age-group, types of status group which have slightly hierarchical or invidious overtones.

5. *The band assembly.* On certain ceremonial or emergency occasions, the entire band may assemble under the leadership of the hereditary band chief and the three councillors or 'second chiefs,' functionaries instituted by the Euro-Canadian fur trade.

It is safe to say that every Peel River Kutchin, during the past century and more, has taken part in each type of grouping. A qualification must be made for the trapping party, in which, as shall be seen, participation has been sporadic during recent decades.

The duration of groupings in any one category may vary considerably, and the membership may differ from one occasion to the next. Thus, two particular nuclear families may pair up every season during the course of many years, or just once and never again. A family may spend one winter in a meat camp and another trapping alone or with a small group.

6. *The local group.* There is a feature of Peel River social organization which stands somewhat apart from the foregoing in that, on the one hand, only a small minority of the band has ever participated in it, and on the other, that it is much more stable than other extrafamilial aggregations. This is the local group, a set of closely related families, ranging in number from four to eight, which constitutes a bilocal extended family.⁵ The family maintains a relatively permanent settlement in a particular locality within the band habitat.

Such a group appears to retain identity and association with a locale for a generation or two.

In the 1930's and 1940's there were three local groups in Peel River territory. The stability of the local group does not preclude individual members from joining other groupings; moreover, a majority or all of the group may remove to Fort McPherson for a sojourn during the three band gatherings at Christmas – New Year, Easter, and midsummer. However, throughout much of the year, most members of the local group maintain residence and undertake the entire program of Peel River Kutchin subsistence and domestic activities in the territory which, by right of occupation and use, is recognized as appertaining to the group.

THE INCEPTION OF WILLIE LAKE'S PARTY

It is evident that in terms of the foregoing classification the group which I was invited to join in early 1947 was a trapping party. Like many others of its kind, the party had been formed rather inadvertently and by accretion. Willie Lake had not participated in a trapping party for several years prior to this, although a few years previously he had been leader of a meat camp and had also led an all-male caribou hunting party.

This winter, he had intended to follow his usual marten-trapping practice, going upriver accompanied only by his wife and elder son, or by a single adult male companion, preferably Stephen John, his eSiDaHu, or another man, his ySiHu. Between Willie and both men the term 'my brother-in-law' was employed reciprocally, overriding the generation difference in Stephen's case. Stephen John and Willie Lake were of an age and of almost equal prestige, although their prestige derived from rather different qualities and accomplishments.

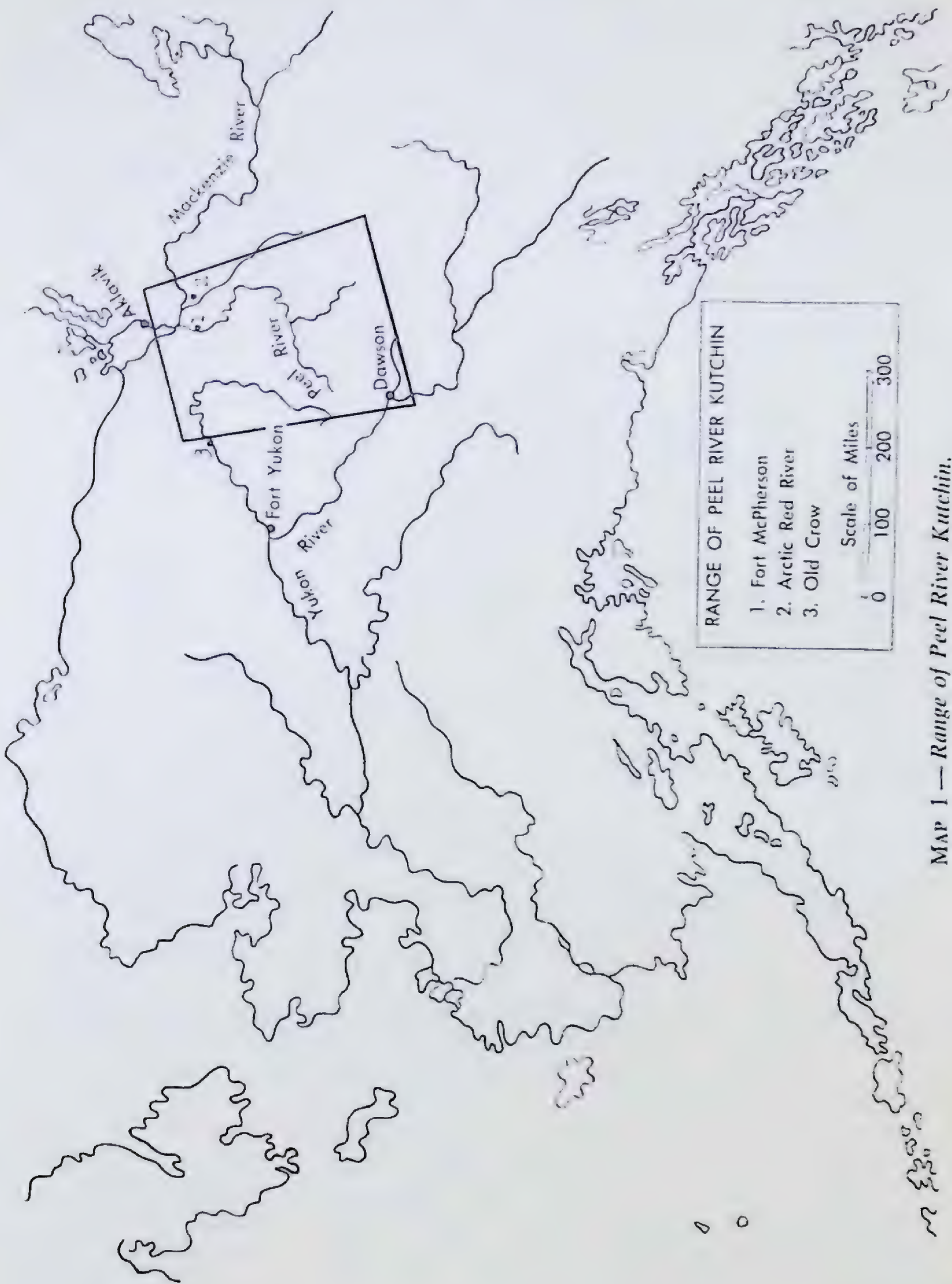
During a visit to Aklavik in the summer of 1946, Willie had been requested by Tommy Delta, a Peel River man living in the Mackenzie Delta, to take his son Tom into the upriver marten country. The high price of fur during the war and early postwar years had lured Aklavik people into the mountains in unprecedented numbers, but few of the younger Aklavik men had experience in the upriver terrain or in marten trapping. When it became known that Willie had agreed to take Tom, other inexperienced young men had asked to go along. Willie then decided to take his wife and younger child and to form a trapping party, which was subsequently joined by several experienced men and women. One of the last to join was Stephen John, whose participation was welcomed by all and who at once assumed, by common understanding, the position of secondary leader.

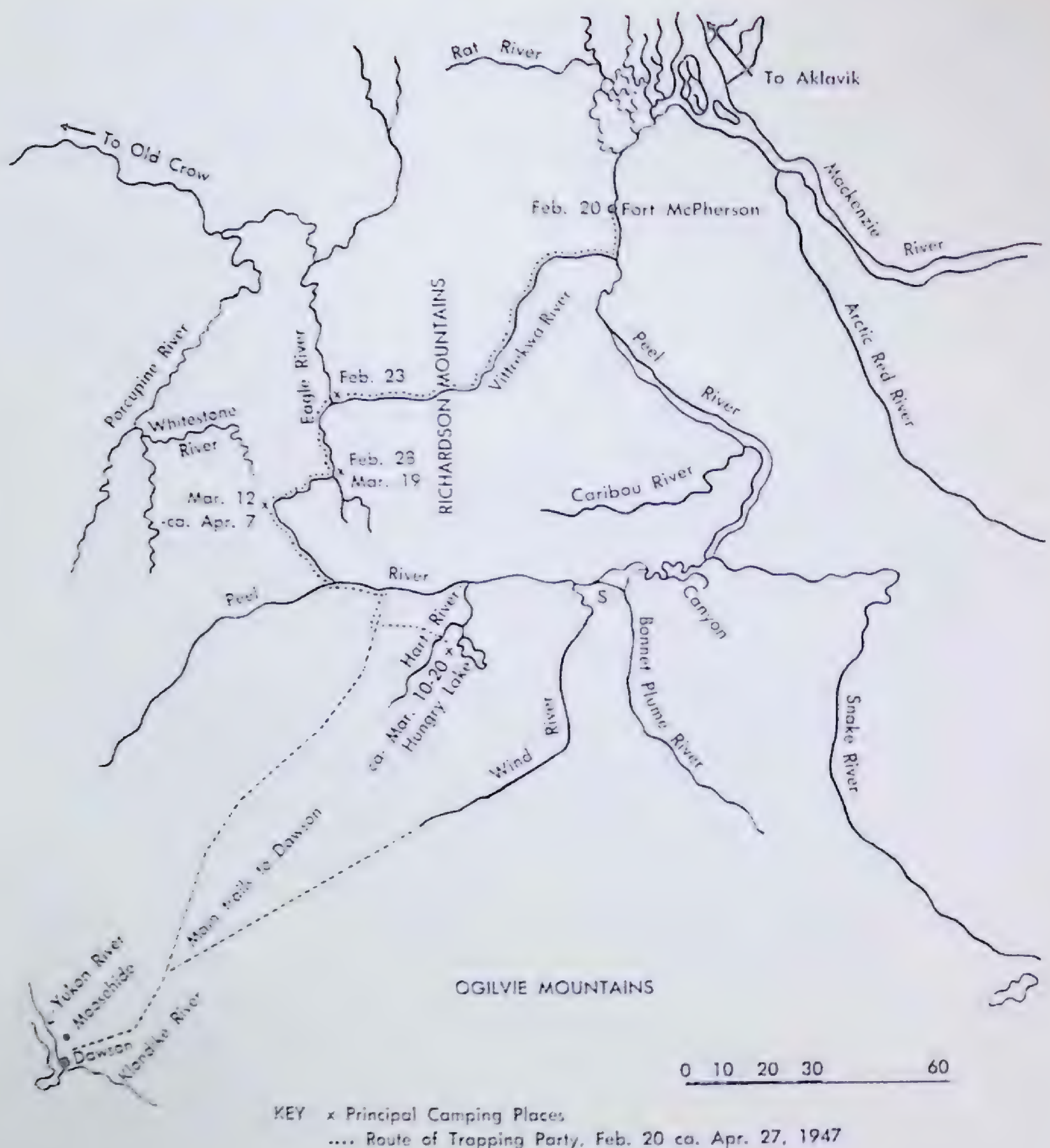
EARLY ITINERARY OF THE PARTY

Seven dog teams left Fort McPherson on February 20, proceeding twenty miles up the Peel to the Vittrekwa River, then southwest up this tributary into the Richardson Mountains, crossing the mountains above timberline on a course almost due southwest. Travel during the first two and a half days was rapid, bringing the teams to the first shelter of trees on the western slope of the Richardsons. After a larger midday meal than usual, it was decided that several of the men should look for signs of marten, while the women, the children, the old man who was with us, and three of the younger men

would descend toward the Eagle River with the dogs and equipment. That night, camp was made just east of the Eagle.

On the basis of his own and of the other men's observations, Willie Lake decided to turn up the Eagle River. It was known from word that had come to the settlement since the dispersal of the midwinter band ingathering that





MAP 2 — *Peel River Drainage and Environs.*

there were several parties and individual or paired families of Peel River and Crow River Kutchin trapping around the head of the Eagle. The men of our party had seen evidence that marten had moved in that direction, preceded and followed by humans.

The marten is semimigratory, particularly under certain conditions when the population is high, as was the case that winter. The movement of marten populations is sporadic, irregular, and apparently in small family groups. Estimation of its future course requires much experience and careful judgment. If the estimate is successful, making it possible to 'trap ahead' of the animals, chances of a large catch are favourable."

Progress up the Eagle was very slow, due to the ruggedness of the terrain and the increasing intensity of the cold.

The party was now fairly in the heart of the Richardson Mountains. As one climbed the slope on either side of the river, the eye was met by the white frozen crests of a mountain world — albeit in miniature. If altitude were all, the Richardsons would be insignificant; the peaks hardly exceed five thousand feet; the passes are for the most part below two thousand feet. Height of latitude, however, makes up for the deficiency, so that the massif rises above timberline, is clothed in snow, carries glaciers, and is at times belabored by storms in which nothing unsheltered can live.

There were no storms at this time, however. The cold, which seemed to intensify daily or even hourly, appeared to preclude strong winds. Such, at least, is Kutchin belief. The cold passed the point where experienced and hardened hunters remarked upon it, passed the point where the wood of axhandles and toboggans shattered under stress into chips like glass, and reached the point where frost appeared to be nipping one's eye-balls as well as other points — the extremities, ears, noses, and cheekbones. Most of us noticed difficulty with vision after being outside for a few minutes. This, coupled with the widely-documented difficulties in judging size- and space-relationships in the Arctic during the Winter above timberline, was an added factor in rendering travel impracticable for several days. For two days the party camped in a relatively sheltered location.

A slight alleviation of the cold spell sent us on our toilsome way. The able-bodied men, leaving camp long before daylight each morning, ranged the hills on either side of the river in two parties, under Willie Lake and Stephen John, looking for signs of marten and of meat animals, and setting some traps. The rest of the group brought the dogteams and impedimenta along the river. This was very hard work. In many places the river ice was broken and piled up; there were snags and large boulders to contend with; sometimes the dogteams had to take to the shore, which presented other difficulties of steep banks and twisting trails. Frequently it was necessary to hitch several teams together, aided by a number of humans pulling on lines.

On the evening of the third day's travel upriver, the eighth day out of Fort McPherson, Willie Lake called a meeting in his tent, attended by all of the active trappers and by several others. Willie expressed the opinion that the party might as well make camp for a while; i.e., more settled camp than an overnight halt. He suggested a move one half mile eastward from the river. If trapping should prove unsuccessful after three or four days, the party would strike camp and move farther upriver.

Two teams of trappers were agreed upon at this meeting. Willie, his son Paul, Robert William, Jr., and Tom Delta formed one; Stephen John, Andrew Venitho, and Clement Nash, the other. Trapping territories were assigned by Willie who discussed them in detail with Stephen. The territories extended up slopes or into the bush from named points and included the mouths of creeks, islands, a lobstick, and striking geomorphic features such as high cut-banks, landslides, exposed black or red strata, and burning lignite beds. (Not all these features were exemplified on this occasion; these are the types of landmark used in distinguishing trap-lines where a group of trappers work in close proximity.)

Since in this party the junior members were ignorant of the terrain and of the habits of marten, only one trap-line was made by each party, with Willie and Stephen demonstrating and then directing the work. It was understood that any marten obtained in the first few days belonged to Willie and Stephen.

Stephen brought a marten back on the evening of the following day, the first day's intensive trapping. It had been caught in one of several traps which he had found set by people who had passed along the Eagle several weeks earlier. Amazingly, the pelt was intact. Stephen did what any passing trapper would do; he skinned and stretched the pelt, thereby saving it from destruction by wolves or wolverines.

Stephen would probably in the course of the Winter come across the owner of the trap which had caught the marten. If not, he would sell the fur at Easter and give or send the proceeds to the owner when it was learned who this was

On the third day at this location, Willie remained in camp repairing his dog-harness and making stretchers for pelts, of which he had brought very few. Moses Nithitzik, the old man, went out with Clement Nash and Robert William, Jr., returning to camp at midday.

That day brought the party up to the complement it was to maintain for a month thereafter (as the Delta people joined us) . . .

On the morning of the fourth day at this camp, Willie made an assignment of the newcomers. Helen and Luke Whaleboat and Tom Delta made a party; Edwin Doke joined Willie's unit. (Slobodin 1962: 48-50, slightly revised)

Mark Lawrence, another Aklavik resident, but one highly experienced in mountain work, ran his own trap-line.

PARTY MEMBERS

'Willie Lake's bunch,' as the party was termed in frontier English—the Kutchin term is very similar in meaning—seemed to be unusual for the rather high average of competence and intelligence of its members.

During most of my stay, the party was composed of twenty-four individuals (excluding myself), including five nuclear families. They were grouped as follows, each group sharing a tent.⁷

1. Willie Lake, age 52. Martha Lake, age 44, his wife. Paul, age 14, Willie's son by a previous marriage; two-year-old son of present marriage.
2. Stephen John, age 50, Willie Lake's eSiDH; Andrew Venitho, age 24, Stephen John's and Helen Whaleboat's SiS; Laura Venitho, age 20, Andrew's wife; two small sons of the Venithos.
3. Edwin Doke, age about 25, Laura Venitho's SiH; Tom Delta of Aklavik, age about 30, Stephen John's MSiSS and Laura Venitho's FSiS; Alice Delta, age about 25, Tom Delta's wife (her father was a white trapper and her mother was Willie Lake's adoptive sister's daughter); three small children of the Deltas.
4. Clement Nash, age 22, son of an English trader who died in the mid-1930's and of his Peel River Kutchin wife; Julia Nash, age 19, Clement's wife and Willie Lake's SWySi; two-year-old daughter of the Nash's; Robert William, Jr., age 20, Julia Nash's eBS; Moses Nithitzik, age about 70, Julia Nash's FeSiH, Laura Venitho's FgodF, and Stephen John's father's "partner."⁸
5. Luke Whaleboat of Aklavik, age about 40, son of an Eskimo man and a Peel River Kutchin woman (his late first wife was Clement Nash's elder sister); Helen Whaleboat, age 43, Luke Whaleboat's wife; Stephen John's sister (her late first husband was Clement Nash's MeB); Stanley Whaleboat, age 11, Helen's son by her first marriage, hence Clement Nash's MBS (now the adopted son of Luke Whaleboat); Mark Lawrence of Aklavik, age about 35, Luke Whaleboat's MySiS, Julia Nash's FyBS.

Willie Lake, a Crow River Kutchin, was born in the Yukon drainage area. As a youth, upon the death of his parents, he moved eastward to the Mac-

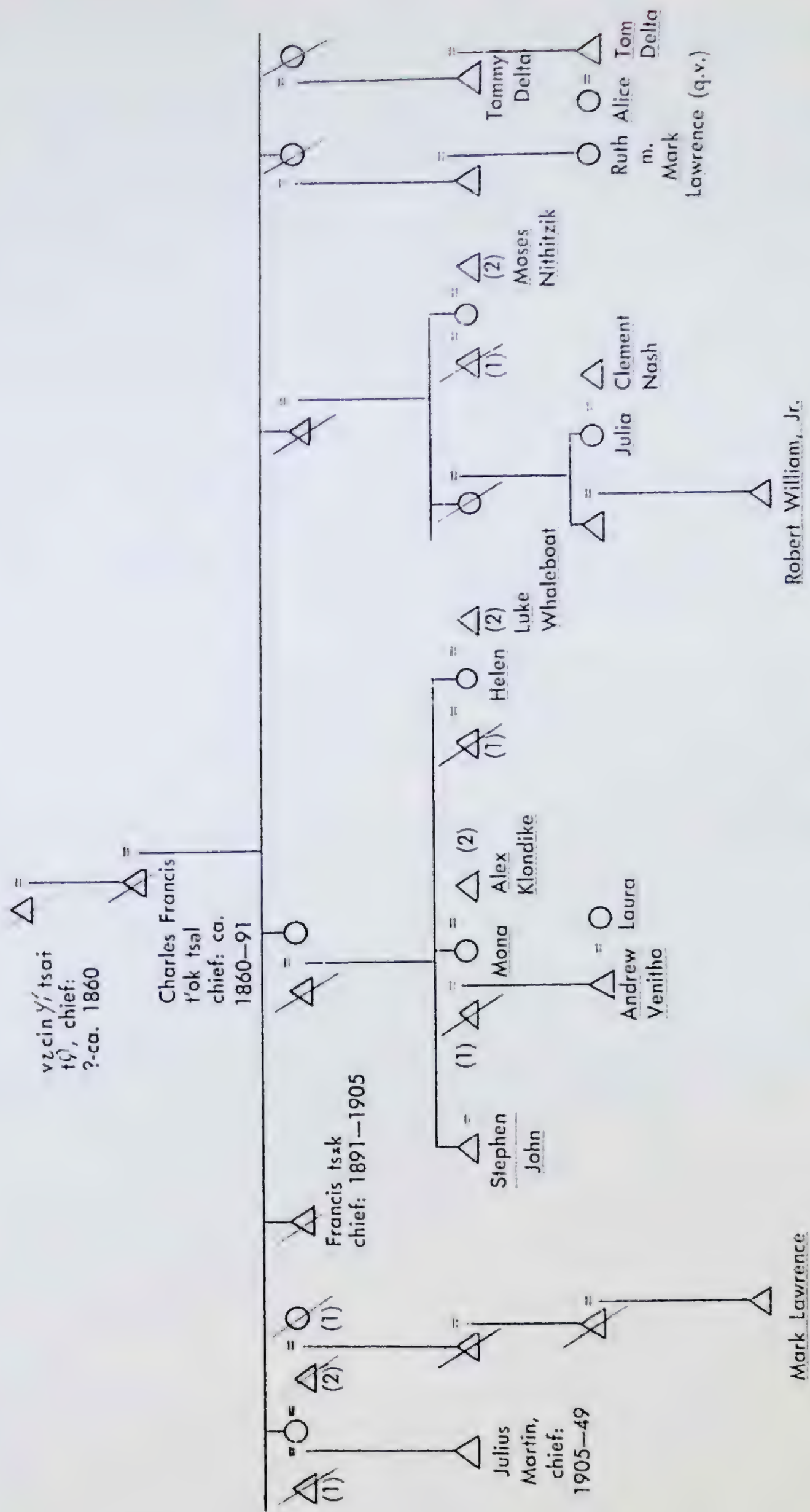


FIGURE 1 — Partial genealogy of descendants of vicin'itsai t'i and t'ok tsal, with some affines. Pseudonyms of members of the Willie Lake trapping party are underlined.

kenzie area. While still a very young man, he rose to a position of leadership in the Peel River band.

The Kutchin have clearly conceptualized the qualities requisite to leadership in their society. A *sine qua non* for leadership is above-average competence in economic pursuits. In addition, a proper balance of 'hard' and 'soft' qualities is necessary. 'Hard' qualities may be designated as shrewdness, drive, and a touch of ruthlessness; the 'soft' qualities are generosity and concern for the common weal as defined in Kutchin culture. Willie Lake was well endowed with these qualities and, apparently, with the proper balance of them. He was, it is true, considered somewhat deficient in eloquence, an accomplishment valued in leaders; his public speaking was reckoned adequate but not elegant or moving. Eloquence, however, although appreciated, is not indispensable for a Kutchin leader.

In the 1920's, during his early manhood, Willie was elected a 'second chief,' or 'tribal councillor,' of the Peel River band. This position was instituted by Euro-Canadian fur traders during the late nineteenth century and confirmed by the Canadian Government in the Treaties of Adhesion concluded with northern tribes (Canada 1921, 1922). Since 'second chiefs' were chosen primarily for their ability to deal with and act for the whites, some of them have exerted little influence within the community proper. However, Lake was a man of influence. On various occasions during 1946-7, twenty-five Peel River men, ranging in age from 21 to about 70, were asked to name the five men whose advice and leadership they valued most. Willie Lake placed fourth in the consensus. The other two tribal councillors did not place among the first five.

Further definition of leadership among the Kutchin is provided by the fact that Stephen John, 'lieutenant' of the party, was not a community leader. Stephen was one of the best-liked men in the band and, indeed, one of the most popular and highly respected persons in the lower Mackenzie area. He was a Deacon of the Church of England, principal native assistant to the Anglican minister at Fort McPherson, and in periods when there was no incumbent minister he served as pastor of the Anglican church in the settlement. Despite his semi-clerical position, he was a favourite among the irreverent younger men and girls for his gaiety, love of fun, and understanding of young people. A practical joker of note, he was even known — in violation of custom — to joke with his mother, of whom he was very fond.

Although Stephen was a competent hunter and trapper, his duties with the church, the delicate health of his only son, and the distaste of his wife for life on the trail confined him for the most part to a fifty-mile radius of Fort McPherson. This in itself would have restricted the exercise of leadership. In addition, Stephen was notably deficient in 'hard' qualities; indeed, he showed no interest in leadership except for the conducting of prayer meetings, which he performed with quiet assurance.

In the limited degree that ascribed rank prevails in eastern Kutchin society, that of Stephen John was high. He was one of eight surviving grandsons of a distinguished nineteenth century band chief who was himself a son of the chief at the time of white contact. Julius Martin, Peel River Chief from 1905 until his death in 1949, was the eldest of these men, a set of brothers

in Kutchin terms, who formed the core at this time of a prestigious descent group, to which further reference will be made.

Willie Lake, Stephen John, and Moses Nithitzik, and also Alex Klondike who joined the trapping party later, shared an incidental status which carried some prestige; they were 'Dawson Boys.' The 'Dawson Boys' were those male band members who in boyhood or early manhood had been drawn into activities centring around the Klondike gold rush. From 1899 until World War I, many Kutchin as well as members of other tribes had been attracted to the Dawson area as guides and meat-hunters, and had taken part in the fascinating, profitable, and hazardous life of the gold rush (Slobodin 1963).

Before Willie and Martha Lake married in 1937, both were widowed. Martha Lake, a strong, energetic woman and veteran of mountain travel, prior to her first marriage had been one of the most active young women of her time in hunting and trapping. Domestic duties and the sharp sexual division of labour enjoined upon married people with minor children had for some years kept Martha from any such activity except muskrat hunting in the Delta. Like other active women, she looked forward to a time when she would be free again to hunt and to trap fine fur. Although she had no opportunity to do so with this trapping party, she was, to a large extent, the trail leader of those who transported children and equipment while the party was on the move.

Moses Nithitzik had been an Anglican catechist since the beginning of the century. I believe he was the senior Anglican official, lay or ordained, in the lower Mackenzie region. This in itself was some indication of high status, since in the late nineteenth and early twentieth centuries the lay church leadership had been drawn as far as possible from the ranks of community leaders. Although Moses was no longer able to do much trapping or hunting, except for rabbit snaring — at which he was assiduous — he was an asset to the party. His intimate and detailed knowledge of the terrain and his fund of anecdotes and tradition concerning each landmark on the way were of some practical value to the party and also, it seemed, served to reinforce and enrich in younger party members the Kutchin feeling for their mountain region.

Moses was in good physical condition for a man of his years; nevertheless, the presence of a septuagenarian on a long-range trapping party was unusual in the modern era. Once, in conversation with several of the young men, Moses referred to his motivation for joining the party.

"You wanted to see the mountains. I wanted to see them one more time."

"How did you actually get to go along?" I inquired.

"Willie asked me. He knew I wanted to go for a long time."

Luke Whaleboat was a member of Aklavik frontier society. His mother's parents had been among those Peel River Kutchin families that had moved into the Mackenzie Delta at the turn of the century, attracted by trade with American whalers at Herschel and Barter islands (*v. inter alia*, Stefansson 1921: 388-9), while the majority of the band had been drawn to the Yukon side of the mountains, oriented toward Dawson. Isolated from their own people, some members of these families had intermarried with Eskimo; their offspring retained a Delta and coastal orientation. Luke Whaleboat, child of such a union, had resided in or near Aklavik during most of his life,

recurrently employed by various Euro-Canadian enterprises there. He was, however, by no means a 'post native' in any pejorative sense. A strongly built six-footer, Luke was a hard-working, responsible person of good repute. Like all Delta people, he trapped and hunted muskrats intensively in the spring and early summer, and he had made many short trips to hunt meat in the hills west of Aklavik when caribou were there. However, he had little experience in trapping fine fur and had never been up the Peel beyond Fort McPherson. In venturing into the mountain country he counted heavily upon the guidance and help of his wife, Helen.

In the winter of 1938-39, when I first heard of her, Mrs. Helen Lobstick, then in her mid-thirties, was married to a Peel River man twenty years her senior. This marriage was a stormy one, for Helen's alleged infidelities were numerous and notorious. It was said that she treated her husband with a high hand, and the colourful stories about her presented the picture of a hell-cat. It was, therefore, with some surprise that, on stopping in one afternoon at the Lobsticks' camp on the Rat River, I found myself taking tea with a matronly person of courteous and dignified demeanor.

A few years after this, Helen's first husband died, and she married Luke Whaleboat. By 1946, no scurrilous gossip was attached to her. Even in the days of her notoriety, no one had denied that Helen was a hard worker and a 'tough traveller,' a term of high praise. She had spent much of her youth in the mountains and was said to know the country and to understand marten trapping.

Mark Lawrence, born and raised near Dawson, was of a Peel River family but had never lived on the Peel. He had moved to Aklavik in the early 1940's. As an experienced mountain man, he was decidedly a 'tough traveller.' A few winters previously, he had made his way alone by dog team from Dawson to Aklavik via Fort McPherson, a trip arousing much comment because Kutchin very seldom travel alone and because Mark had made his way through severe storms, breaking trail for his dogs in deep snow for a couple of hundred miles. Upon arrival at the camp of a local group, one hundred miles up the Peel, he and his dogs were near exhaustion, but "he managed," as the Kutchin say. During another Dawson-Aklavik trip, this time with his wife and child, Mark took motion pictures of the journey because, as he put it, "It should be interesting."

Respected for his intelligence and competence, Mark was considered rather moody and unsociable. His solo trip across the mountains was cited as evidence of this.

Although several members of the party, like many members of the band, were of known white ancestry, Clement Nash was the only party member of Métis or 'half-breed' status.¹ The five surviving Nash brothers, with their mother, their wives, a sister and her Kutchin husband, and the children of these couples, formed a local group twelve miles down the Peel from Fort McPherson. It was a family strongly marked by temperament: difficult, prickly, yet possessing considerable charm. The brothers quarrelled fiercely among themselves, yet they formed a united front toward others. The family cohesion as well as its marginality with respect both to Peel River and to Delta frontier society was signalized by their residence together on the lower Peel. The Nashes were among the Métis who had achieved limited acceptance

in the Indian society. It is likely that the early death of their father and the strong personality of their mother had been decisive in turning their interests and abilities in considerable measure toward Kutchin activities.

The young men and women who made up the rest of the party's adult complement need only be noted briefly.

Robert William, Jr., a lanky youth, was the son of a highly successful Peel River trapper. He showed promise of approximating the Peel River ideal by combining an easygoing manner with a capacity for hard work and what was considered good luck — in earlier years it would have been called power — in hunting and trapping. His nickname in English, 'Happy Way,' referred both to his disposition and his luck.

Andrew 'Tex' Venitho, who had been employed for a season by the Army at Norman Wells during the War, had brought back some mannerisms, preferences, and souvenirs which had given rise to his nickname. As closely as that of any Peel River man, his manner exemplified the 'cowboy swagger' of the marginal ethnic in the Canadian north to which Dunning has referred (1959: 122). Indicative of the difference between the acculturation situation of the Peel River Kutchin in 1947 and that of the communities described by Dunning were the relatively inconspicuous behaviour of 'Tex' and the fact that his G.I. indoctrination did not prevent him from putting in a good deal of time hunting, trapping, and fishing. In this he was greatly aided by his wife, Laura, whose conscientious conduct as wife and mother had fairly won her the status of respectable woman. In their mid-teens, a few years previously, she and her sister Mona had been considered promiscuous.

Laura Venitho had hoped that Mona would join the party with her husband, Edwin Doke. This had not happened. It was only after Stephen John and I had returned to Fort McPherson from the mountains that the reason for Mona's absence was forthcoming. Willie Lake had made it clear, Stephen said, that Edwin might join the party only if his wife stayed behind. The impression was given that Mona had not succeeded as well as Laura had in living down her earlier reputation.

Edwin Doke, Dawson-born, and Tom Delta of Aklavik, both of Peel River parentage, were the least competent men in the party. Edwin frequently complained of stomach trouble and indeed did seem to suffer from an intestinal disorder during this period. Unlike other members of the party, he and Tom were quite unhappy in the mountains and yearned for the Delta country. This attitude was not shared by Tom's wife, Alice, who had been brought up close to the Lake family—Willie called her "my daughter"—and had spent some time in the mountains as a girl. Indeed, Martha Lake remarked confidentially that Willie had accepted Tom's company largely for Alice's sake. The Lake family's affection for Alice played a part in initiating the trapping party.

It is not to be inferred that Edwin and Tom were utterly incompetent or unwilling. There are a very small number of Peel River persons who are so characterized. Their lot is unenviable; objects of almost universal contempt, not only are they miserably poor by the standards of a society that is not affluent, but they have no opportunity for improving their condition. The great majority of Peel River men and women conform, at the least, to a

standard of competence sufficient for minimal participation in and profit from a trip such as that in which we were engaged.

The big boys, Paul Lake and Stanley Whaleboat, as well as the smaller children, appeared to enjoy themselves thoroughly and without exception, barring minor mishaps. Paul Lake did almost a man's job in hunting and trapping. Stanley accompanied his parents while his mother, advised at first by Stephen John, set out traps and taught her husband how and where to catch marten. By the end of the season's trapping, Stanley had caught a marten and a fox.

FURTHER COURSE OF THE PARTY

After nine days at the camp on the Eagle River, only two marten and four fox had been trapped. Meat was scarce; no caribou had been seen and only one moose had been killed.

A meeting of the men was held in Willie Lake's tent, at Willie's insistence. Martha Lake and Helen Whaleboat were present. It was agreed that since the marten were moving about very little due to the cold weather, they had better be sought elsewhere. There was much discussion of possibilities in various directions. It was Helen Whaleboat who, speaking in an undertone to Moses Nithitzik, first mentioned the upper Whitestone River. The old man, evidently concurring, proposed this goal to the company at large, and eventually they accepted. Next day, most of the traps were picked up by the men while the women and older children, with Moses and 'Happy Way,' prepared for striking camp early the following morning.

During the move to the upper Whitestone area, the men remained closer to the dogteams than they had done on the trip up the Eagle, in order to be able to lend help when necessary and to speed travel. The party reached the upper Whitestone in two days, then turned almost due south toward the upper Peel. On a height of land separating the Whitestone, the Peel, and the east branch of the Porcupine, a paired family of Crow River Kutchin was encountered. The Peel River party had tea with these people, who also served caribou soup and small helpings of meat from a few caribou killed not long before. Our party supplied some biscuits and tinned preserves.

The Crow River people had been in the mountains since early January and were about to leave for the settlement of Old Crow with a good catch of marten. They reported that trapping conditions were fairly good and that there were signs of caribou not far away.

The party made camp near the willows of a mountain creek and immediately set out to hunt caribou. Mrs. Lake and Mrs. Whaleboat with their sons went on the hunt. When evidence of fifty to seventy-five caribou was seen, Willie Lake divided the hunters into two sections, under Stephen and himself. The plan, as is usual in such a situation, was to surround the caribou, who were browsing in a narrow valley. Stephen, with the youths and younger men in his charge, was to make a wide circuit along the hills and come down into the other side of the valley, but not directly opposite Willie's group. When this was done and his men were in place, prone or kneeling, Stephen held his rifle vertically over his head. At this, Willie opened fire, and the rest followed suit.

Over thirty caribou were killed and immediately dressed down. Willie and Stephen were not completely satisfied with the result of the hunt as they

felt that an opportunity had been missed to secure enough meat for humans and dogs until the end of marten trapping. As it was, another hunt would be necessary. Unfortunately there were not many other caribou signs in the vicinity, and the survivors of this herd had scattered and would move fast for a day or two.

The inexperienced men were now trapping on their own, with occasional advice from the veterans. As trapping in the area proved fairly successful, and an occasional moose or stray caribou was obtained to maintain the meat supply, the party remained at this location until early April. In the latter part of March three men left; Stephen John wanted to return to his tubercular little boy, Mark Lawrence was worried about his wife who was ill in hospital at Aklavik, and Edwin Doke had by then admittedly had enough of the mountains and wished to rejoin his family and prepare for early muskrat trapping. I left with these men.

Shortly before our departure the party was joined by a sixth family, that of Alex Klondike, his wife Mona, and their six-year-old son. Alex Klondike, grandson of the most famous Kutchin shaman of the middle Yukon, was a leading trapper and part-time trader who alternated residence between Old Crow, Y.T., and Fort Yukon, Alaska. Mona Klondike was the mother of Andrew Venitho by a former marriage. When Stephen left, Alex Klondike assumed second position in the party. That he would do so was a foregone conclusion, not only because of his prestige, ability, and intimate knowledge of the terrain, but because of his adeptness in those minutiae of Kutchin etiquette which, it is felt by eastern Kutchin, are obsolescent in the Peel River band but are observed by Crow River people, many of whom were in the area and might be encountered at any time.

Early in April the party moved southwest toward Hungry Lake, camping near another trapping party of about twenty persons, composed of families from several Kutchin bands. The two parties remained distinct, although there was a good deal of visiting between them. There was also one joint caribou hunt directed by Willie Lake, Alex Klondike, and Peter Fish-trap, a Peel River man who was the primary leader of the other party.

During the third week in April both parties disbanded, and individual and family members returned to their home settlements of Old Crow, Aklavik, Arctic Red River, and Fort McPherson. They returned to sell fur or to settle debts at the trading posts and thereafter to join the muskrat hunting already underway in the lowlands.

STRUCTURE OF THE GROUP

A number of sociologists have discerned distinctions between what G. C. Homans terms the 'external' and the 'internal' structure of the primary or small group (Olmsted 1959: 105; Homans 1950: *passim*; cf. White 1949: 81). Such a distinction is useful here, in that the 'external' relationships of the trapping party, relationships to the physical and to the societal environment, do not strictly precondition the structuring of inter-relationships among party members. Considered in terms of external and internal structuring, the position of the trapping party among extra-familial Peel River groupings has been discussed elsewhere.

In terms of their external systems, the Peel River trapping party, meat camp, fish camp, and trading party may be classed together as occupational groups in that they are formed primarily to serve the economic purposes implied by the names given them in this study. The local group then stands by itself as a miniature community, associated with a defined habitat and concerned with the total problem of living within the territory.

On the other hand it is quite clear that these five types of group . . . fall into another categorical arrangement. In both structure and function, the meat camp, fish camp, and trading party are very similar, while the trapping party and the local group resemble each other in many respects. At times, for instance, one is tempted to think of the meat camp and fish camp as seasonal variants of the same social phenomenon — they are not quite, however. On the other side of the picture, if the Alexis (local) group were to pick up stakes and go trapping *en masse*, it would make a good trapping party, while if Willie Lake's party had been able to settle down somewhere on a semi-permanent basis, it would almost have constituted a local group — again one must say, not quite.

The classification of occupational groups in one category with the local group in another is in terms of the external systems of the groups. The second classification is in terms of their internal systems.

The trapping party and the local group are approximations to bilateral extended families led by one who is, or who is treated as, a kinsman *primus inter pares* on the basis of relative prestige within the group, prestige based primarily upon sagacity in subsistence activities. (Slobodin 1962: 75)

It should be clear from the story of its inception that membership in the trapping party was voluntary. The party as constituted had never existed prior to this time and in subsequent years never came into existence again. In this sense, the party conforms to the common sociological definition of an association: individuals come together to seek a common or shared goal, or to promote a common interest. Such "relatively short-term groupings of persons 'localized for specific work tasks'" (Helm 1965: 378) have been termed action groups (Freeman 1961: 213; Blehr 1965), occupational groups (Slobodin loc. cit.), and task groups (Helm loc. cit. and her Note 17). The last of these terms, though leaving something to be desired, is probably the most suitable for use in discussions of Northern Athapaskan society.

While these twenty-four persons formed a kind of association, i.e., a task group, they also constituted the semblance of an extended family. In addition, all but three adult members of the party were members of a descent group with some claim to corporate perdurability.

Fourteen of the seventeen adult party members were affiliated with or married into a nonunilinear or cognatic descent group.¹⁰ Seven of the nine children in the party were also members of this group. The group was comprised of the descendants of Charles Francis t'ok tsəl ("Small Nipples"), successor in the band chieftainship to vicin^yitsai t'ɛ ("Father of Him Whose Face is Painted Properly"), chief at the time the first whites ascended the Peel in 1840. All his sons except t'ok tsəl died young, without male issue. The accompanying chart shows those descendants relevant to the present discussion.

This type of descent group is of some importance in Peel River social life. During this century, at any rate, only a minority of the community has been able successfully to claim membership in one; the groups are in some degree prestigious. Each group stems from an eminent person or an eminent pair of siblings. An additional and apparently necessary condition for the estab-

lishment of the group is the existence of a set of successful and relatively long-lived siblings in the second generation. Kutchin cousin terminology is Hawaiian (Murdock 1949: 223); siblings include first cousins.

To the particular descent group involved in the Lake trapping party have belonged all known hereditary chiefs of the Peel River Kutchin. The group is known in Kutchin as *hakaia nət* ("the chiefs' ilk"), and Kutchin in speaking English sometimes refer to it semifacetiously as "the royal family," although most cognatic descent groups persist no longer than three generations. This group has persisted as a recognizable entity for four generations, and there is some likelihood that it may survive further, under greatly changed conditions. There is reason to believe that the group's durability and some of its prestige derive from the existence, in the generation of Chief Julius Martin and Stephen John, of a set of sixteen siblings, of whom nine are shown on the chart. There were eight men (*see* p. 65) and eight women. Several were very able, and almost all were high-spirited, long-lived, and mutually loyal.

It may be noted that Mark Lawrence was not genealogically connected to the group, although his wife was. He claimed membership, however, asserting that his FaFaFa was Chief Julius's father. That he should make such a claim was evidence of the group's prestige; that the claim went unchallenged was an indication of the regard in which Mark was held.

In 1947 there were four cognatic descent groups in existence among the Peel River Kutchin, of which one was moribund as a corporate entity; that is, the sentiment of cohesion and identification among its members had become attenuated. Including the identifiable membership of the weak group, the descent groups in 1947 comprised about 75 persons out of the total population of 375.

It need hardly be said that among themselves members of the Lake party did not use kinship terms denoting their actual genealogical spacing and relationship. Terms for the more remote relationships may be elicited, but so far as I know they are never used in ordinary social intercourse. I have heard them only in historical stories and legends. As Osgood has stated, among the Kutchin, "except for close kin, people are addressed according to simple categories based primarily upon age" (1936: 117). Outside the nuclear family, the categories most commonly employed are grandfather/grandmother-grandchild, elder-younger brother, elder-younger sister, and either of two forms of 'friend,' one of which implies greater distance and respect than the other. The principal difference between the kinship usage of party members during the trip and at other times was a greater differentiation of terminology during this period, on the part of party members who were not close kin. This difference is not surprising in view of the increased volume and intensity of interaction among fellow-members of the party. The same dynamism is observable in the total kinship system, producing finer distinctions in terminology and behaviour among close kin than among more distant relatives.

The terms assumed for the duration of a trapping party by members who were not primary or secondary kin (Murdock 1949: 94-5) are listed below. Terms for which there were no referents in Willie Lake's party are in parenthesis.

grandfather
 (grandmother)
 father's older brother
 (father's younger brother)
 mother's older brother
 mother's younger brother
 mother's sister
 'youthful kinsman'

older brother
 younger brother
 older sister
 younger sister
 sibling's child
 grandchild
 brother-in-law
 sister-in-law
 friend

Although there are terms in Kutchin for father's older sister and father's younger sister, I was unable to ascertain any instance in which these were used by trapping party members for women who did not already stand in these relationships to the speaker.

It has been stated that in its internal structure the trapping party approximated an extended family. Actually, it resembles the Kutchin local group, which is an extended family, more closely in function than in structure. The functions of the local group have already been described (Slobodin 1962: 54-7). Information obtained about all four trapping parties operating during the winter of 1946-7 indicates that the internal structure and the economic functions of the trapping party differed in certain respects from those of the local group or extended family.

1. There was no single framework of relationships which held for all members. Thus, Julia Nash called Moses Nithitzik 'my mother's elder brother.' Julia was classed as a sibling to her age-mate Robert William, Jr. (genealogically her eBS). Robert William, Jr., called Moses 'my grandfather,' as did Clement Nash, Julia's husband. Alice Delta called Willie Lake 'my father.' I have no record of her husband, Tom Delta, addressing or referring to Willie other than by name. Tom Delta called Stephen John, who was genealogically his FaMoeSiS, by the term 'my father's elder brother,' while his wife, Alice, called Stephen 'my mother's elder brother.' The closest genealogical link between Stephen and Alice had four intervening relationships, which is too tenuous to be employed as a kin bond by Kutchin under ordinary circumstances.¹¹ In other words, they were not related, in Kutchin terms. Subject to tacit or expressed acceptance by his seniors, each adult member created a terminological kindred around himself, so that in terms of communication patterns, the party sounded like a collection of overlapping kindreds.
2. The above-mentioned terminological pattern was qualified by the fact that age-mates and juniors were usually addressed and referred to by name. Some low-ranking junior adults used names rather than kin-terms for high-ranking persons.
3. 'Brother-in-law' and 'sister-in-law' were the only affinal statuses ascribed specifically for use in the party. It happened that in Willie Lake's party there was only one instance of a pre-existing close affinal relationship of any other kind, that between Mona Klondike and her son's wife. There is in Kutchin a term differentiated by the criteria of age and speaker's sex for each of the secondary affinal relationships.

4. In Kutchin society, where an extended family functions as a social unit, leadership is usually invested in a pair of brothers, a term which in Kutchin includes all sons of siblings; or, less often, the leaders are a pair of brothers-in-law. Leadership of the trapping party is also dual, but in a minority of known cases the leaders were brothers or brothers-in-law. Moreover, the leaders are not assigned positions vis-à-vis the other party members similar to those occupied by the leaders of the extended family; e.g., father, older brother, father-in-law, father's brother, mother's brother, grandfather.
5. The economic functions of the extended family are bounded by a consistent framework of kinship ties; e.g., proceeds of trapping are shared to a large extent among the group as a whole in terms of kin obligations. In the trapping party the proceeds of trapping are not shared outside of each constituent nuclear family. Members of the trapping party do engage in cooperative effort (e.g., meat hunting) and in sharing (e.g., of meat), but these activities are largely functions of the external structure of the party; that is, they are ways of dealing with ecologic and economic necessity and are not closely related to the obligations of kinship. The assumed kin ties may be invoked in connection with these activities, but only, so far as I was able to observe, for honourific or jocular purposes.

BROTHERS-IN-LAW

Among adults it was the person of higher status who assigned the relationship to be employed during the trip. The person addressed then responded with the appropriate term. Thus, Stephen John decided to call Mark Lawrence "my brother-in-law" while they were together in the mountains. Usually he called Mark "my younger kinsman" or, less often, "my son-in-law"; the latter because, as Stephen put it, "he is my brother's son-in-law." The father of Mark's wife was mother's sister's son to Stephen.

In Kutchin society, the brother-in-law relationship is avowedly ambivalent. On the one hand, brothers-in-law are frequently partners and close associates; on the other, there may be a strong element of hostility and distrust between them, illustrated by the local name for the voracious wolverine, the trapper's enemy, known, especially in the mountains, as "our brother-in-law" (i.e., the 'greedy one'). "Brother-in-law" is a joking relationship. Like such relationships in many other societies, the behaviour associated with it may range from warm and friendly to aggressive and invidious, depending upon personality factors and the mutual esteem or disesteem of the kinsmen involved.

Stephen continually joshed Mark throughout the trip. On one of the coldest mornings, the sportive deacon pulled Mark out of the Whaleboats' tent in his sleeping bag and rolled him in the snow. Unlike some of Stephen's genealogical brothers-in-law — not members of the party — Mark responded mildly or not at all.

After we had returned to Fort McPherson and Mark had gone on to Aklavik, Stephen explained why he had ascribed the brother-in-law relationship to Mark and had joked with him so heartily. Mark, he said, was a good man and his friend, but "he has a sad way." When misfortune struck, Mark

was not resilient, "He just gets quiet and goes away from the people." He, Stephen, realizing that he had the gift of laughter in the face of trouble, had tried to make Mark laugh, to take his mind off of his troubles — "and maybe I did for a while."

It was evident that Stephen succeeded, at least on one occasion, in this benevolent effort.

On the sixth day of camp on the middle reaches of the Eagle River, Mark found the tracks of a moose, which was subsequently hunted and killed. The meat was very welcome, for our small supply of 'proper' meat — moose and caribou — was nearly exhausted. Following Kutchin etiquette for the situation, Willie Lake, killer of the moose, presented it to Mark as "spotter." Mark, also following custom, returned half of the carcass, split lengthwise, to Willie, along with the entire skin except that of the head and legs. He then gave the head to Stephen as 'lieutenant' of the party. Willie and Mark carefully apportioned most of the meat among the heads of family on the basis of family size, counting the unattached men as dependants of the families with which they were tenting.

Two weeks later Mark located moose sign again, and shot the animal. By this time the party was well supplied with meat; hence the atmosphere at the division of the game was more relaxed and jocular than on the earlier occasion, which had been a rather solemn affair. Tom Delta, who was credited with killing the animal, presented the moose to Mark, who duly returned half the carcass, as well as the skin, to Tom. Mark then gave the forelegs to "my mother's brother," Willie Lake — his mother, a Crow River woman, was distantly related to Willie — and the hind legs to "my grandfather," Moses Nithitzik. This was a twofold compliment. Mother's brother and grandfather are predominantly respect relationships; moreover, presentation of the legs carried an implication that, as mature hunters of proven ability, neither man needed to concern himself with the interdiction against eating the legs of moose and caribou, and especially against cracking the long bones, which was formerly one of the many restrictions on the diet of immature or weak hunters. The tabu has little practical force in the modern culture but retains symbolic force as a traditional referent. In Kutchin society, as in many others, the relationship between grandparent and grandchild generations is mildly jocular. There was an overtone of humour in Mark's gift to the old man, of which Moses indicated his appreciation by brandishing his moose leg as a club and making as though to chase several of the young women, shouting the war cry, "*Onjit!*"

To all appearances the moose had been completely distributed, but nothing had been given to Stephen John. At this point Mark raised his skinning-knife in a gesture strikingly dramatic for so quiet a man, and declared, "Now for my brother-in-law." Reaching into the interior of the split carcass, he slashed and tugged until he had pulled out a yard of the extreme lower end of the large intestine. He passed this, dangling on his knife and still steaming, to Stephen. There was great laughter at this. 'Tex' Venitho shouted *tsæt snɛ·kai nʔainzɪt*, at which the laughter redoubled, for this was an opposite play upon words, a form of wit much favoured by the Kutchin. It could be taken to mean, "There's feces for you!" or "You have an Eskimo here!"

The relevance of the latter reference lies in Kutchin abhorrence of Eskimo food habits.

Not to be completely outfaced, Stephen gravely accepted the intestine, remarking that he was lucky to have a brother-in-law who remembered how much he liked soup. He had used the head of the first moose to make soup; now he would make gut-soup, which he intended to share with his brother-in-law.

Stephen John had three other brothers-in-law in the party; however, in these cases the relationship was not temporary. Two were his sisters' husbands, Luke Whaleboat and Alex Klondike, and the other was Willie Lake. Between Willie and Stephen there was little jocularly; Willie was not much of a jokester in any case. Rather, there was an easy familiarity developed through years of close association in work and community affairs. When the two of them hunted together, few words passed; each knew what the other was likely to do. Mild jocularly prevailed between Stephen and easygoing Luke Whaleboat, but I observed none between either of them and Alex Klondike during the few days between the arrival of the Klondikes and my departure. Neither Stephen nor Luke were well acquainted with the western Kutchin man. Willie Lake, who knew Alex Klondike well, usually called him *tʷəl θE*, literally "hardened bone," i.e., goalkeeper in the Indian version of Association football; Alex had been notable for his stalwart playing of this position. On a few occasions Mark Lawrence, who knew him well, and 'Tex' Venitho, his stepson, also addressed Alex Klondike in this manner.

JOKING IN THE PARTY

Since antiquity, philosophers have been concerned with the meaning of wit and humour. Following the leads of Bergson and Freud, there has in modern times been some study of the function of humour in personality dynamics. Social scientists, however, have given little systematic attention to humour in its relation to social structure and social organization. Ethnological discussions of 'joking relationships,' especially those influenced by Radcliffe-Brown (1952: 90-116), present a striking contrast to the neglect by ethnographers and social anthropologists of humour in other contexts. Such neglect is largely justifiable since it is notorious that humour is the last aspect of a culture that an alien can learn to understand.

These comments apply to my observations in the trapping party. There was much joking and laughter, but I was not, at the time, looking to a sociological analysis of it. Notes and recollection suggest that jokes were initiated for the most part by (1) the leaders, Willie and Stephen, (2) persons of lower status who were esteemed, such as Martha Lake, Helen Whaleboat, Luke Whaleboat, "Happy Way," and—on the occasion noted earlier—Mark Lawrence, and (3) persons outside of the rank order, such as older children.

It is not clear to me whether Moses Nithitzik, whose frequent joking was sometimes appreciated, sometimes ignored, fell into the second category, as a respected elder, or into the third, as a person no longer economically effective. Andrew Venitho attempted jokes now and then, but his efforts received little attention by the group as a whole; as a result he usually

addressed his jokes to his age-mates only. Clement Nash might have been included in the second category, but he indulged in little humour. His semi-marginal status may have had an inhibiting effect, for back at the Nash homestead and in the settlements he sometimes participated in joking.

I have no record or memory of Tom Delta or Edwin Doke joking, and if the women other than Martha and Helen did, it was not in the company of the men.

These generalizations appear consonant in part with Rose Coser's observations on joking among colleagues on the staff of a mental hospital: "Those who were in a higher status position more frequently took the initiative to use humor" (1960: 95). However, in the trapping party it was not true that "the target of witticism, if he was present, was never in a higher authority position than the initiator" (ibid.). In the rigidly hierarchical hospital staff, Coser observed that "the status structure is supported by downward humor" (op. cit.: 86). In the trapping party, humour may be said to lend support to the status structure; its patterning undoubtedly reflects authority and ranking, but there are other considerations. Joking was sometimes directed at Willie Lake and Stephen John. Here, it is probable, may be discerned "the release of aggression in a witty manner, [serving] . . . to prevent the undisguised outbreak of hostility or the bottling up of frustrations" (op. cit.: 95).

"Laughter Among Colleagues" is cited because the relatively uncomplicated situation and Coser's penetrating analysis of it provide a good illustration of some social functions of humour. To the great contrast between the structure of the mental hospital staff and that of the trapping party must be added a cultural contrast. Humorous self-deprecation is important to the Kutchin. It is closely associated with the Kutchin ideal of a man or woman as a person who is highly competent without showing it any more than necessary, outside of particular ceremonial occasions. Humorous self-deprecation is recognized and approved in our own society; or more precisely, it is an acceptable alternative among several styles of behaviour in our society. Among Kutchin such behaviour is quite strictly enjoined. Rather than an alternative, it is almost the only possibility.

Except, as noted above, in certain ritual situations, and then only in a highly stylized manner, a Kutchin must not indulge in overt verbal self-validation. The approved style for verbalizing self-satisfaction or a claim to distinction is by inversion, by humorous and often transparent self-deprecation. To use a simple example, a man who is asked directly if he has had a good catch of fur or kill of caribou will reply in the negative: "I couldn't catch (or hit) a thing. I'm no hunter." He knows, of course, that his questioner will soon learn the true state of affairs, whatever that may be. By the same token, a person of high status and prestige may endure a great deal of joking, receiving it with good grace and as a sign of his worth, apparently on the principle reported of James J. Corbett (1866-1933) when he was world heavyweight boxing champion. Asked why he had disregarded the rudeness and jostling of an aggressive drunk, 'Gentleman Jim' replied, "I can afford to."

The targets of Kutchin joking are persons of high and markedly low prestige. The style of witticism does not appear to be conditioned by its

direction, upward or downward.¹² Just as in other societies, Kutchin joking functions as a socially non-disruptive expression of hostility, both toward those to whom one owes deference and toward those of whom one disapproves and who by their very presence as failures or deviants may call into question the legitimate system of values and rewards. However, a distinction is made. Kutchin witticism directed at the high-placed is regarded by all as a mark of esteem and a means of reducing the social distance between jokester and target. This is certainly not true of joking aimed at the lowly.

WILLIE LAKE AND THE 'ROYAL FAMILY'

It will be recalled that three of the party's adult members were not members of the quasi-corporate kinship group descended from nineteenth-century band chiefs. Of the three, one was the low-ranked Edwin Doke, and the other two were no less than Willie Lake and his wife Martha.

During the past decade or so, there has been considerable interest in what Goodenough called nonunilinear descent groups (1955: 72), including groups termed 'kindreds,' with one or another kind of qualification. Freeman's essay on the kindred brought to attention that "one of the notable characteristics of bilateral societies is the formation of temporary kindred-based action groups" (1961: 213).

It is tempting to view the Kutchin trapping party as kindred-based. Genealogically, the descent group can be seen as a series of overlapping kindreds, or, if one accepts the view that the kindred excludes affines (Freeman 1951: 201-2), the chiefly descent group may be seen as a series of groups based upon what Blehr calls the kith relationship (1963: 270-1). This perspective would relate to the analyses by Freeman (op. cit.: 211-5), Pehrson (1957: 81-99 and *passim*), Goodenough (1962), and Helm (1965) of the recruitment of organized kin-groups in cognatic or bilateral societies.

As noted earlier (p. 70), party members addressed and referred to one another to a considerable extent as though they constituted a collection of interconnected kindreds. In fact, however, the dynamics of group formation discussed recently by Freeman and by other students of bilateral social structures did not apply here. That the party was not recruited in terms of kindreds is indicated in the first step of its formation. There is no question that the party was initiated by Willie Lake, yet no member of the party except his three primary kinfolk belonged to Willie Lake's kindred. It is true that he and Stephen John have an affinal link and call each other brother-in-law, but this is an exclusively dyadic relationship. No one considers Willie to be affinally connected to the chiefly descent group as are Alex Klondike and Luke Whaleboat.

In addition, it cannot be supposed that recruitment to the trapping party spread, through overlapping or nodal kindreds, from Stephen John, a key figure in the descent group and a long-time trapping partner, to Willie Lake. Far from being an early member of the party, Stephen John was one of the last to join it; it appeared until the last that illness in his family was going to prevent his going. In point of fact, the party, as a trapping party rather than a family or paired family or partnership, began when Willie Lake agreed to take Tom Delta, which he did to oblige Tom's father, Tommy, and because the Lakes were fond of Tom's wife, Alice. It would be absurd to suppose

that any subsequent recruits to the party joined because of a distant connection with Tom, an inexperienced young man of no great repute.

Aside from children accompanying their parents, and wives their husbands—in the case of the Whaleboats, it may be said that the husband accompanied the wife—only two members of the party seem to have been motivated by *a priori* considerations of kinship. Although the aged Moses Nithitzik had been specifically invited by Willie Lake, and might have accompanied Willie in any case, he was, or so he told me, encouraged to go by the presence of so many of “my people”; actually his wife’s “people.” Alex Klondike, as a Crow River man, was partially moved to join this Peel River party, rather than another, because his wife’s brother, sister, and son were members. By contrast, the younger men and women, although aware of the ‘royal family’ as an entity, had not noticed that the party consisted for the most part of members of this group and were mildly surprised when this was pointed out while we were camped on the Eagle River.

Older and younger trapping members joined the party because they were anxious to get upriver in the season of high fur prices, with one party or another, and because they knew Willie was a good party leader.

From the point of view of the total community, rather than that of conscious motivation for individual action, the preponderance of the chiefly descent group in this trapping party is not wholly fortuitous, as there is a slight but noticeable tendency for members of this and the other extant descent groups to interact more frequently within the group in economic pursuits, rather than outside of it. The point, however, will not be urged here. Of the four trapping parties active during the 1946–47 season, one other besides Lake’s was dominated numerically by members of a particular cognatic descent group: six or seven out of eleven adult members of the trapping party (see Table 1). Thus two of the four trapping parties were each representative of a quasi-corporate descent group, while two of the four descent groups in existence at the time were each actualized, in part, by a task group.

That a leader may be in some degree alien to the group is not particularly unusual for the Kutchin. Several of the better-known chiefs of the Yukon

TABLE 1

Some Comparisons Among the Four Trapping Parties Upriver During the 1946–47 Season

	Number of members			Total	Number of adult members identified with a predominant cognatic descent group	Duration of trip, weeks	Relationship of dual leaders
	Aged 16+		Younger than 16				
	M	F					
Party 1 (Lake) . . .	11*	6	9	26	14	10	Brothers-in-law
Party 2 (Fish-trap)	7	4	7	18	6-7†	12	Uterine brothers
Party 3	6	4	8	18	—†	7	None
Party 4	9	6*	9	19	—†	ca. 11	Formal partners

*Party 1 had one man aged about 70 (Moses Nithitzik), and Party 4 had one woman aged 67 or 68. Neither of the other parties included anyone over 60 years of age.

†Party 3 included two persons from one descent group and three from another. Party 4 included three from one of the descent groups represented in Party 3. The Fish-trap brothers, leaders of Party 2, were among the six or seven party members from one descent group. In addition, Party 2 included two persons from the ‘chiefly’ group, which predominated numerically in Party 1.

Kutchin during the past century have been Kutchin from other bands or, in three cases, originally non-Kutchin speakers. On the Mackenzie side, a leading figure and 'second chief' among the Arctic Red River Kutchin in recent decades has been a Slave from the Fort Norman area. If tradition may be credited, alien leadership is not a novel, post-contact development. In Kutchin mythology and in the legendary war tales, as in many Old World hero tales, the heroes became leaders of communities to which they were alien.

TRAPPING PARTY LEADERSHIP AND SMALL-GROUP THEORY

The party comes into being at the initiative of the principal leader; moreover it is his responsibility to plan the itinerary, direct meat hunts, allocate trap-lines, decide upon location of camp-sites, and summon meetings where future plans are discussed. In addition, he is responsible for the instruction and guidance of tyros who have been accepted in the party.

The extent to which party members are consulted varies with the leader; Willie Lake, though firm in making decisions, was given to consultation, probably because he had with him several experienced people. No Kutchin leader can be arbitrary, since membership in his following is voluntary within the limits set by economic and ecologic necessity. The group members can, and sometimes do, 'vote with their feet' by moving away. This is true for every grouping from the nuclear family to the community at large.

In Kutchin society, almost all status is achieved (Slobodin 1960: 131), and furthermore most statuses, including kinship, are to some extent situationally conditioned. This conditional limitation characterizes leadership, especially of the smaller types of grouping, where leaders are active participants. For the larger groupings such as fish camp, meat camp, and band assembly, the leadership, which is to a considerable extent administrative rather than participant, is less subject to situational change.

Success in the past as a trapping party leader does not provide status as a party leader in later years. Moses Nithitzik had been a trapping party leader, but it would not have occurred to anyone in 1947 that he should be one. Principal leader and 'lieutenant' may exchange roles if circumstances arise where some special skill or experience of the latter is particularly relevant. Such exchanges are rare, but they have occurred. The opportunism of Kutchin social organization has limits; however, no case was known where a rank-and-file party member was elevated to leadership during the existence of a particular trapping party.

It has been noted earlier that the Kutchin have designated and verbalized the qualities requisite for leadership, the 'hard' and the 'soft' qualities. Both Willie Lake and Stephen John were able trappers, 'tough travellers,' and responsible members of the community, but Willie unlike Stephen was also endowed with the necessary 'hard' qualities. A similar distribution of traits occurred in the leadership of another trapping party of the 1946-47 winter, wherein Peter Fish-trap, the primary leader, was quite tough, albeit mild-mannered as a rule, while his 'lieutenant' and younger brother, Edward Fish-trap, was a less incisive personality. Several other competent men who were not 'hard' had been 'lieutenants' in trapping parties but had not been primary leaders. On the other hand, Alex Klondike, who succeeded Stephen John as 'lieutenant' to Willie Lake, was reputed to be a 'harder' character than

Willie. Moreover, the occasions whereon primary leader and 'lieutenant' exchanged roles suggest that the combination of 'hard' leader and 'soft' 'lieutenant' is at most a tendency, rather than an overt or implicit rule.

Much of sociological small-group theory has been concerned with a distinction between what R. F. Bales has called the 'social-emotional' and the 'task' areas of group social life (1950: 9ff.). Bales has gone on to assert the incompatibility of the two types of function. On the basis of experiments with American university students in groups of three to six individuals, he concludes that the task leader is most disliked by other members of the group and that the emotional leader is least likely to be a task leader, and vice versa (Bales 1953: 111-61). Other sociologists have sought to show tendencies toward the reconciliation of 'task' and 'emotional' functions (e.g., Blau 1960; Blau and Scott 1962).

The Kutchin 'soft'-'hard' dichotomy indicates awareness of a distinction that is parallel to 'emotional' and 'task' functions; 'hard' qualities are valued not in themselves, that is, for their social effect, but because they are deemed necessary to the accomplishment of tasks. However, any such formulation, either that of the Kutchin or that of the small-group theorists, is too schematic to be fully applicable to the actual dynamics of Kutchin group life.

A Kutchin is well acquainted with all members of his own and neighbouring bands. Even if they have not met, as some members of the Lake party had not met Alex Klondike, they are all well known by repute. The conduct of a Kutchin, in all but a few unusual cases, holds few surprises for his fellows. If agreement to join in a trapping party is reached, prospective leaders and party members are well aware of what to expect of each other. The leaders will be neither excessively overbearing nor ineffectual, for if they were, these characteristics would be well enough known to make it impossible for such persons to gather a party. Those accepted into the party will have a reputation of being capable of doing their share toward the accomplishment of group tasks and of refraining from disrupting the social-emotional area. The exclusion of Mona Doke from Willie Lake's party was an example of concern for the latter qualification.

In comparison to the membership of extrafamilial small groups in our society, in an analogous Kutchin group, there is a narrow range of social expectation and a distribution of capacity and experience wherein as a rule only a few members differ widely. In our society, Blau asserts, group members

... will be motivated to improve their own qualifications [and] ... may also exchange services with one another instead of receiving them from one with superior abilities in exchange for deference. Such practices threaten the position of the informal leader or leaders of the group. (1960: 556)

Among Kutchin, on the contrary, such efforts are taken for granted and are regarded as lightening the task of the leader. The expectation of rank-and-file self-help does not warrant relinquishment of leadership, nor does the expectation of limits in the members' autonomy augment the authoritarian component of leadership. These issues have been to a large degree implicitly settled long before the formation of a particular grouping.¹³

The power, although not necessarily the authority, of any Peel River Kutchin leader is limited by the physical and moral freedom which allows his followers to leave him at almost any time. Peel River Kutchins are well aware of the contractual basis for membership in any grouping larger than the nuclear family. In the trapping party, certainly in modern times, this is emphasized by the fact that the major activity and *raison d'être* of the party is not a cooperative venture. Within the party, the trapping is carried out by individuals or families. Aside from the special duty, necessitated by recent conditions, of assigning tutors to party members inexperienced in mountain trapping, the leader's only concern with the trapping of other party members is in suggesting geographical points from which they run their trap-lines, thus keeping trappers out of each other's way. This must be accomplished by mutual agreement, without any fixed leader, as sometimes occurs in a small group of trappers travelling together who may set out short trap-lines on their way to their respective regular trap-lines.

It is in the ancillary but indispensable cooperative activities of travelling, moving camp, hunting for meat, and making plans for moves that the direction of the leaders functions fully. For the active trapper member of the party and for their wives, the stated purpose, that is the principal purpose given by them, for joining a trapping party rather than venturing into the mountains alone, is that it facilitates travel to a fine-fur area, and that while living there it is advantageous to subsistence. The function of the party in this regard is quite similar to that of the chartered plane or boat or sled, flown by a bush pilot, which transports a trapper, his dogs, toboggan, traps, and several months' supplies to a desired location. It must be apparent by now that the full function of the trapping party and the attitude of its members toward the group are by no means so purely economic as the comparison would suggest. The fact remains, however, that the cooperative activities wherein leadership is manifest are subordinate to trapping. If these activities, either by their excess or their inefficacy, are felt to interfere upon or to hamper trapping, any able-bodied man feels free to withdraw. That such withdrawals, although possible and far from scandalous, are rare, appears to be evidence for the relative efficiency, from the Peel River viewpoint, of the trapping party. It is also indicative of the significance of the non-economic functions of this type of grouping.

NON-ECONOMIC FUNCTIONS OF THE TRAPPING PARTY

To the Kutchin, the trapping party is a satisfactory working arrangement, and Willie Lake's party was considered to be successful in this regard. In terms of Western ideas about efficiency, the trapping party is not a very high-grade production unit. Its size and composition are not optimal for speed and efficacy in travel, subsistence, and trapping. Probably the best unit for these purposes would consist of two experienced men, which is what Willie Lake had originally planned, or again eight to ten men. Considering the economic value of women to the larger group, for moving camp, setting, preparing meals, repairing clothing, etc., three or four women might be added. However, Willie Lake's party, whose size and composition were greatly increased among its twenty-four native members nine months later, consisted of one man, five women, one fourteen-year-old youth, and eight *Wahgichish*.

If the question is raised as to what the party members other than those principally engaged in trapping, hunting, tending camp, and transporting goods were doing on the trip, it could be answered that they were living. Indeed, the same answer might be made for all party members; moreover, it was clearly understood that they were living, as I had been told prior to the trip, in the way "the Indians really live," or rather, in the way that they feel they should live.

Even in modern times, oriented as the Peel River people are toward the settlements and downriver activities, it is to a Kutchin no waste of time and effort to bring his small children into the traditional mountain home of his people. Many Kutchin are quite conscious of and articulate about benefits to be gained from such an experience. Important among these is learning to live "like an Indian," as they put it in English, in a healthful and active manner.

A function of the trapping party is to raise the prestige of its members or to maintain it if already high. Until very recently a Kutchin man or woman could not expect to be very highly regarded if he or she had not participated in the trapping of fine fur and had not sojourned in the marten country.

One evening in the Nashes' tent, Moses Nithitzik was telling stories. I remarked, at the conclusion of one tale, that I had heard a slightly different version from Old Murray.

Happy Way laughed. "Don't pay attention to what that old man tells you. He doesn't know anything."

I asked him why not.

"Why, he's never been anywhere but those five miles up and down the river from his cabin to the Fort and back. He's never been in the mountains."

"Is that right?" I asked Moses.

Moses shook his head. "Murray is a pretty lazy man. It's true he's never been anywhere much."

Like other northern hunting peoples, the Kutchin set a high valuation upon the physical mobility, which is a necessity for them. For the Kutchin, who are ardent sightseers, to travel to see new things is good in itself. Especially valued is the title of 'tough traveller,' one who can make his way through difficult terrain and hardships.¹⁴ However, extensive and arduous journeys along the Arctic Coast or down the Yukon, accomplished by some Peel River people, do not quite equal travel through the mountains at the head of the Peel drainage, their traditional homeland.

THE MARTEN COUNTRY

When I joined the trapping party I had acquired enough of the Kutchin language to follow and participate in conversations about everyday matters. As we progressed into the mountains it seemed that I was losing this limited knowledge. What had happened was that the vocabulary and to some extent the syntax of my companions' speech had been changing. Upon my return to Fort McPherson I discussed this with two observant, bilingual half-breeds who were mountain travellers. They agreed that they had noticed such changes.

"Probably Old Moses and Stephen and Willie and maybe Martha and Helen started it," remarked one. "You see, all those English words and

French and broken Slavey and Husky,¹⁵ all those words that have gotten into Loucheux [Kutchin] around McPherson and Aklavik, somehow they don't use them so much when they go into the mountains. We use the real old way of talking, as much as we can."

"Then again," the other man pointed out, "there's a lot of different things to talk about up there that you don't usually talk about down here—like all the different places, and the stories about them, and the way the marten live, and the way the snow lies, which is different up there. And you run out of store grub so you don't say, 'Open a pot of jam.' . . . And you might use old-time medicines. Didn't Simon make Indian tea and things? . . . Sure."

An additional consideration suggested was that if one met Crow River Kutchin, which was likely, one would want to "talk right," as these people speak a "purer" or a more old-fashioned form of the language than do the Peel River people.

Another readily observable change, previously indicated by one of the observers quoted, was in food habits. It was true that our supply of store-bought food became very low. Not much had been brought along, except for flour, tea, and sugar. After a month the flour was gone, and the supply of sugar was almost exhausted. We were never hungry, however, for there was always meat. During one period of seventeen days the party subsisted entirely on caribou and moose meat. Almost all parts of the animal were utilized in the cooking. I was told that this was not a particularly lengthy period for an all-meat diet on a prolonged trapping trip. Although there was an adequate store of imported tea, some party members preferred 'Indian tea' brewed from several local herbs. Some of the children were given a brew of willow-bark, which Edwin Doke also drank for his intestinal complaint.

These changes in diet were viewed with satisfaction by the older members of the party and by several of the younger. Not surprisingly, Kutchin are very concerned about food, which in one connection or another plays an important part in most conversations. A dependence upon imported food products purchased from traders is considered to be a major factor in an alleged decline, both physical and spiritual, of the eastern Kutchin during recent generations, and in the still greater degeneration that some neighbouring communities are felt to have suffered. The danger of alien foods was a recurrent theme in addresses by the chief and other elders at band assemblies; the theme was also voiced frequently by senior members of the trapping party.

"It's true," remarked Happy Way one evening, stretched at his ease in the Nashes' tent, "homebrew and canned stuff is no good. But how can you keep away from them? It's like women . . ."

The traditional home of the Peel River people, the mountainous area at the head of the Peel, is a storied region. To the outsider a remote wilderness, to the Kutchin it is the old home country. Almost every noticeable geographical feature is named, and there are many localized myths and historical traditions. This is much less true of the downriver area, where there are, to be sure, quite a few geographical names along main trails, but the associations are for the most part post-contact and even quite recent. Willie Lake's

party passed a number of places where awesome events had occurred in times past, where strange creatures had lived and — who knew? — perhaps still did live in deep lakes or within the rocks.

Clearly, the prestige value of travel and work in the marten country is a feature of modern times. Residence in and knowledge of this region were taken for granted as an essential feature of existence for most Peel River people from pre-contact times until the onset of the demographic changes set in motion by the great rise in muskrat prices during World War I. These have been outlined elsewhere (Slobodin 1962: 36–41).

The anthropologist's interest in cultural processes, his scrutiny of those possibilities and limitations in cultural history that are called cultural (or social) evolution, must derive from and remain rooted in the specific realities of human behaviour, in the actualities of social action, and in other cultural manifestations. As Arensberg has observed, in the consideration of "a social arrangement, a culture trait, an institution . . . the first thing to know about it is how it operates within itself and how it came to be. Only then can one ask what its functions are now, were once, and may yet be" (1957: 109). To do otherwise is to generalize upon the basis of invalid data or, a more insidious error, from culture-bound abstractions concerning thought and action. In the paper cited above, "Anthropology as History," Arensberg points out that anthropology, combining the longest of human time perspectives with a close scrutiny of behaviour patterns, provides a distinctive kind of insight into the study of the human past.

Our discipline is preconditioned to derive specific motivations, whether 'economic' or otherwise, from such arrangements [patterns of interaction] rather than from abstract human nature or needs. But it also sees these patterns and arrangements as historically achieved rather than as the consequences of the play of general forces of human nature or circumstance, as the economist might view them. Anthropology expects the social arrangements underlying economic behaviors and motives to be particular human inventions of historical times and places. It expects social and economic inventions, like technical and artistic ones (of which we have a better record so far), to have spread and combined in diffusions, evolutions, and convergences and to continue to do so today and in the future rather than simply to prove responsive to the effects of universal processes of association, as sociology sees them. This processual bent of anthropology has substantial implication. . . . It makes of the interpretation of non-Western data a vitally historical, even 'culturological' pursuit rather than a psychological, economic, or sociological one. (op. cit.: 100–1)

NOTES

1. This paper, dedicated to Conrad Arensberg, was originally drafted in 1961. An excerpted form was presented at the Conference on Band Organization. The paper has been further revised for publication.
2. Field work between August 1946 and May 1947 was aided by grants from the (United States) Social Science Research Council and the Arctic Institute of North America.
3. Pseudonyms are used for all persons living in 1947 with the exception of Chief Julius Martin, who died in 1949.
4. These are discussed in Slobodin 1962: 78–83.
5. In an earlier discussion these were termed bilateral extended families, but it would seem that Murdock (following Kirchhoff) is correct in saying that for the extended

family, "Residential terms . . . correctly suggest the crucial differentiating factors" (1949: 34).

6. Recent (1966-68) researches appear to cast doubt upon the alleged semimigratory behaviour of marten (A. Macpherson, pers. comm.). However, the wandering propensity of this animal is a tenet of faith among northern trappers, who plan accordingly.
7. Except for the two infants, the children frequently slept in tents other than their parents'. I stayed with Stephen John and the Venithos most of the time but spent several nights in the Nashes' tent.
8. 'Partner' is a lifelong formal adoptive relationship formed between a pair of children or adolescents of the same sex. Partnership in this sense is far from universal among modern Peel River Kutchin.
9. Forty-seven members of the Peel River band born between 1890 and 1947 were offspring of white men (Slobodin 1962: 67). There were undoubtedly a good many born between 1840, the time of earliest effective white contact, and 1890. These persons have Indian rather than Métis status.
10. To call this set of persons a 'descent group' introduces ambiguity into the usage of the word 'group' in this discussion; all the other types of group to which reference is made consist of memberships which assemble periodically and engage in face-to-face transactions, whereas this one does not. Of course, many types of group in sociological analysis are not primary. We are told that a group is "any collection of social beings who enter into distinctive social relationships with one another" (MacIver and Page 1949: 14), and that "such interaction need not be of the face-to-face variety, nor need all members interact with all other members" (Oliver 1958: 802). Nevertheless, the Kutchin 'descent group' noted here may be, like 'the kindred,' a type of social process or a principle of interaction, rather than a group. I would have been glad to beg the question by applying to it some term already sanctioned in kinship analysis, but I cannot think of one that is applicable. (I tried one, but my fellow-confrères at the National Museum would not let me get away with it.) A suggestive unpublished paper by Thomas Hazard on the development of Northwest Coast local organization has led me to think of the Kutchin descent group as a proto-*numaym*. However, an attempt to justify the use of this term would carry discussion well beyond the scope of this paper and the interests of the conference.
11. Under all but very unusual circumstances, this marks the limit of kinship as defined by Kutchin. Thus Ego's FaSiDS is likely to be recognized as a kinsman, but Ego's FaSiDSS is not. The limit of kinship recognition is also a function of the relative prestige of the persons at either end of the genealogical chain. As Menander remarked, "It's a job to find the relatives of a poor person," "Ἐργον εὐρεῖν συγγενῆ πενίτος ἐστὶν" or, as the Kutchin say, "Respected people have many kin; lowly people do not," "dīnzī kadūn inli vətāzī koⁿli: dīnzī nērtācō kwā." There were genealogical connections between Willie Lake and some Peel River families, but they were too remote to be employed even for this man of mark.
12. My appreciation of Kutchin wit and humour is limited. However, bilingual informants who are themselves adept in the verbal arts have said that style in wit and humour is dependent upon the speaker's skill and is status-irrelevant.
13. On more general grounds than are offered here, Hsu has questioned the cross-cultural applicability of the distinction in small-group theory between 'task' and 'emotional' functions (Hsu 1965: 654). Unfortunately he has failed as yet to explain how one judges which primary kin dyad is dominant in a given society. His argument here seems to be a circular one.
14. The name of the Kutchin culture hero atočòkái ('He Paddled a Different Route') illustrates the main theme of his adventures: perseverance through difficulties along an uncharted and perilous course.
15. Broken Slavey is an obsolete trade jargon formerly current along the Mackenzie River. 'Husky' is frontier English for 'Eskimo.'

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DISCUSSION

EGGAN: Do the Kutchin keep count of the results of these parties and evaluate leaders or talk about them in terms of how many marten their groups bring back?

SLOBODIN: The leaders are not judged by the amount of fur that the party as a whole brings back. They recognize that there are a number of factors involved — one of which is luck. I think that the social relationship of having a 'happy ship' type of group is rather important. On the other hand, if they came back with no fur at all the group would not be very happy.

BICCHIERI: From your description I would suppose that if the leader cannot hold together the group on emotional-social bases it would tend to disperse.

SLOBODIN: Yes, it seems to me that the members exert much control over the leader because they are more free to move in terms of both the ecology and the cultural expectation. There are no particular imperatives involved. It appears to be very rare for members to leave a hunting party, for I found no cases of this occurring. Perhaps one can interpret this to mean "really it wouldn't be done." It might happen if the necessity arose, but in all cases known to me this necessity is obviated at the beginning with the inception of the party, because each individual knows everyone else in the group very well before joining it. It is true that people did not know Alex Klondike in the beginning, but still his behaviour was within the tolerable range. Alex was rather tough, and perhaps some who would not have wanted to serve under him would have left.

HELM: Would you say, then, that in this group the actual focal prestigious figure who mobilized true recruitment is a dead person?

SLOBODIN: Right! This is true of the two nineteenth-century chiefs. I assume here that the 'group' refers to the descent group, not to the trapping party. I was not aware of this relationship until long after I had become a member of the party. So far as I was concerned, it just happened that people joined on the way. Of course, when I worked out the kin structure of other trapping parties I found that they comprised related people to the extent described in my paper.

MARSHALL: You have described an example of the division of a kill. Could you discuss principles that were involved in such divisions?

SLOBODIN: Much is decided in advance in that undesirable people, for instance, troublemakers just are not taken along with the party. In general, distribution is rather straightforward, though at times the situation appears to be congruent with that described by Rogers. He indicated that among the northern Algonkians the leader takes rather a smaller share than the rest, just to obviate criticism. There is a specific etiquette outlined for sharing moose killed by the party. The person that kills the moose usually gives it to someone else. That person then distributes it and is obliged to give himself a smaller part. When two fellows are hunting, each one tries

to jockey the other into killing the moose. Some men who are very skilful hunters make the situation a real chess game, not to let the moose get away and yet have the other one kill him.

McKENNAN: In what way does the local group differ from this trapping party which you have described? In my opinion the two would have been synonymous in the past. In the early days at Fort Yukon it was said (in the accounts of Murray and MacDonald) that "Senati came in with his group of twelve hunters." It sounds as if that would be an operation similar to your hunting party except that the youngsters would not have to be taught because they would have grown up with the group. On the other hand, such a group would have been the same entity as the local group or what I had been calling the band.

SLOBODIN: I think that they are probably close to being synonyms and that the differences were situational.

McKENNAN: The Chandalar Kutchin band led by Christian fits your description on a smaller scale except that that group stayed together all year round; they hunted together; they trapped together; and in so far as they fished, they fished together. It would be a group very similar to your hunting party except that for hunting and trapping it would not differ as yours did.

SLOBODIN: My feeling is that there is a great deal of freedom in the structuring of these groups. People might be asked to join, but they came and went as it suited them. The causes were so multivarious that there was not very much rigidity about recruitment. At the same time there was always the strong possibility of leaving the group without any hard feelings. This might not be so true in a local group.

McKENNAN: Was the fluidity not related to the phenomenon of 'posting' as described by Honigmann, that is, families converging about a post such as Fort McPherson? In the protohistorical period, before the founding of Fort McPherson, would the Peel River Kutchin not have all been grouped together in one place? Among the Chandalar Kutchin, for instance, where none of the people live around Fort Yukon, the local group, the trapping party, and the hunting party would be the same group, even allowing for such fluidity as you mention.

HELM: In the contemporary picture at least there is the local group which is now settled on the land in a cluster of cabins, as well as parties which form out of the fort or bush settlement. The latter sort of group may be formed more loosely. That is, they may be members of the friend category rather than being close kin. These are the impressions that I get from studying the Dogrib and the Slavey, and they seem to be congruent with the Kutchin situation. Certainly the local group is.

McKENNAN: I agree with this assessment. There is an acculturative factor. If one seeks to reconstruct the protohistoric band, the current groups have many features that throw light on it, but there also are many differences.

SLOBODIN: Yes, this [party]¹ is an agent of the historic band [as distinguished from the pre-contact band].

EGGAN: It is still puzzling to me why Willie Lake became the leader of the party whose activities you have described and how he picked out one of the solid blocks of kindred.

SLOBODIN: [These are two of the most crucial questions raised by the ethnographic material present. Because of time limitations, some of the analysis published here which may throw light on these questions was omitted from the Conference presentation.

1. Willie Lake became the party leader because the party grew up around him. It was not the case that a group of related persons got together and agreed to have Willie Lake as their leader. He was joined by individuals and nuclear families, acting severally for the most part. The members joined him rather than going upriver alone because in the particular historic setting it was advantageous for them to join a party. They joined Lake rather than another leader, in large measure because he was a leader and a mountain man of proven ability.

2. As to "how he picked out one of the solid blocks of kindred," it is certain that he did not do so intentionally. As noted in a section of the paper not delivered at the Conference, "there is a slight but noticeable tendency for members of this and other extant descent groups to interact more frequently within the group in economic pursuits rather than outside of it." It is my impression — unvalidated as yet — that what has been called the chiefly descent group, the one represented in the Lake trapping party, is more cohesive than other such groups. There is, in the formation of the trapping party, a sum of individual actions which may be seen from outside the system as comprising a group action. Does it become legitimate to say that the group is acting, i.e., choosing Lake as a leader, or is being acted upon, i.e., being chosen by Lake? The answer to this question is involved in one of the oldest and most fundamental problems of the behavioral sciences, still a very live issue; namely, the reality of collectivities and the nature of group action.]

EGGAN: This [leadership of the party] is a political relationship, and you would expect some of the men in this group to take over leadership in this kind of activity.

SLOBODIN: The senior, the most prestigious man in the descent group — aside from the aged Moses Nithitzik who was *hors de combat* — was Stephen John, and he just was not a leader. So they picked the best leader available. [It must be noted again that this involved a series of individual but socially related choices.]

¹Brackets in this section are included at the request of Dr. Slobodin in order to refer to material not included in the oral presentation of his paper.

ATHAPASKAN GROUPINGS AND SOCIAL ORGANIZATION IN CENTRAL ALASKA¹

BY ROBERT A. MCKENNAN

RÉSUMÉ

Le renouveau constaté dans l'intérêt que l'on porte aux théories sur l'organisation sociale a attiré l'attention sur la civilisation des peuples chasseurs, pêcheurs et pasteurs. Les Athapasques de l'Alaska centrale se prêtent particulièrement bien à ce genre d'études, car, d'une part, ils sont avant tout un peuple chasseur de caribou, et, d'autre part, la période au cours de laquelle on a pu entrer en contact avec eux est si récente qu'il est possible de reconstituer de nombreux aspects de leur culture aborigène.

Chez ces Athapasques, on trouvait une unité sociale de base qui se superposait à la cellule familiale: c'était la bande locale, qui pouvait compter de 20 à 75 personnes. Chaque bande exploitait les ressources alimentaires de son territoire en construisant des palissades pour garder les caribous et des barrages pour capturer les poissons, ce qui nécessitait l'effort collectif de tous les membres de la bande. Les bandes voisines, à leur tour, se groupaient entre elles pour former des bandes régionales auxquelles les premiers écrits donnent souvent le nom de «tribus». Ces bandes régionales, qui parlaient le même dialecte et possédaient la même culture, étaient principalement endogames, en contraste avec les bandes locales qui les composaient, lesquelles étaient surtout exogames.

Dans toute l'Alaska centrale, l'organisation sociale reposait sur une parenté exogame du côté maternel qui comptait généralement trois membres. La terminologie exprimant les relations de parenté indiquait une bifurcation collatérale au niveau de la génération des parents et, dans la plupart des groupes, les termes de parenté au niveau de la première génération suivaient le système iroquois. Les mariages entre cousins étaient la forme d'union la plus en faveur, tandis que la résidence des époux était le plus souvent déterminée par celle de la mère.

An important movement in American anthropology during the past twenty years has been the renewed attempt to discover general principles or 'laws' of cultural and social development. In this field the contributions of Murdock (1949), Eggan (1955), Steward (1955), and, most recently, Service (1962) are infinitely more sophisticated and rest on much better ethnographic data than the pioneer work of Lewis H. Morgan and Edward B. Tylor. Although differing from each other in many respects, these modern theories share one common virtue, they add a much-needed time depth to the essentially ahistorical orientation of Malinowski and Radcliffe-Brown, whose ideas influenced so much American anthropological thought from the late 1920's to World War II. One interesting by-product of this emphasis on the process of culture change and the interrelationship of social organization with other aspects of culture, particularly subsistence activities, has been a renewed interest in the cultures of hunters-fishers-foragers. This interest in turn has stressed the need for a better understanding of the cultures of the American Arctic and Subarctic, because in this region, as Chang (1962: 29) has observed, "the impact of a cultural ecology upon society is more plainly

observable and is less complicated by historical factors than in other areas of the world." The Northern Athapaskans, hunters *par excellence*, play a role, albeit a different one, in the theories of social organization of both Steward and Service. Unfortunately some of their information concerning these Indians is either incomplete or in error. Likewise, as I will try to point out later, some of the inferences and hypotheses these authors make regarding the social structure of these northern hunting bands do not find support in the ethnographic facts.

Since the Athapaskan area is so well represented in the present symposium, I will focus my own observations on that part of this area I know best, a stretch of Alaska running north from the Canadian border to the Brooks Range and including the drainage system of the Tanana River, the middle reaches of the Yukon River, and the drainage of the East Fork of the Chandalar. My field-work extends over a considerable period of years beginning in September 1929 when I spent nine months among the Indians at the extreme head of the Tanana River (McKenna 1959) and continuing in 1933 when I lived for a summer with the Chandalar Kutchin (McKenna 1935, 1965). Two years' service with the United States Army Air Forces in Alaska during World War II provided a few brief occasions to work with Athapaskans including some Koyukon informants from Nulato, Galena, and Kokrines, as well as many opportunities to study the Alaskan terrain from the air. The primary aim of this early field-work was to reconstruct the native culture as it had existed at the time of the first white contact. Since the contact period was relatively late throughout this whole area, this objective was not so unrealistic then as it may seem now, more than thirty years later. My oldest informant among the Chandalar Kutchin had been a boy when A. H. Murray built Fort Yukon for the Hudson's Bay Company in 1847. Many of my Upper Tanana informants remembered well the coming of the first white men, Lieutenant Henry T. Allen and his party, who passed through their territory in 1885.

In 1962 I returned to the Athapaskan field again and covered the entire Tanana Valley from Snag, Y.T., to Tanana, a village peopled mostly by Koyukon natives and located on the middle Yukon across from the mouth of the Tanana. The primary objective of this field trip was to work out the demography and cultural relationships of the various Athapaskan groups inhabiting the Tanana Valley at the time of white contact.² My information regarding details of social organization is admittedly scanty for some of these groups, particularly those of the Goodpaster River, Salcha River, and Lake Minchumina areas, since there were only a few survivors of these bands in 1962.

CONTACT PERIOD

In order to arrive at any understanding of the culture of the area as it was at the time of the first contact, it is necessary to know the following: first, the time of such contact; second, its nature and intensity; and third, the major cultural changes that resulted. Obviously in any area as large as the one under discussion, each of these factors has varied from group to group. For the purposes of this paper they will be discussed only in broad outline. The first white man to live in the area was Alexander H. Murray who founded Fort Yukon for the Hudson's Bay Company in 1847. This post im-

mediately became a trading centre for the Chandalar Kutchin, but because they lived at considerable distance they did not feel the full effects of the fur trade until some decades later. The first Christian missionary, Rev. W. W. Kirkby, visited Fort Yukon in 1861, followed a year later by Archdeacon Robert McDonald. Mission influences like those of the fur trade reached the Chandalar in a somewhat attenuated form; thus by the early 1860's European contacts had affected the middle Yukon and lower Tanana regions. By that time both the Russians, from their permanent post downriver at Nulato, and the English, from their base at Fort Yukon, were making periodic visits to Nuklukayet at the mouth of the Tanana where they traded with the natives from the lower reaches, at least, of that great valley (Dall 1870: 276). There is no published description of the Tanana Valley until Lieutenant Allen's account of his exploration in 1885 (Allen 1887), but several American fur traders, including A. C. Harper and L. N. McQuesten, had visited it briefly by the late 1870's (McQuesten 1952). The early Anglican missionary, Rev. V. C. Sim (MS.), had also made a brief visit to the lower Tanana Valley in the summer of 1884 when he accompanied a group of Indians upriver from Nuklukayet, apparently reaching the vicinity of present-day Fairbanks. Although there were few, if any, permanent white residents in the Tanana Valley until the discovery of gold near Fairbanks in 1901, by the 1880's the Indians of the upper Tanana were trading regularly at Yukon River posts located near the present settlements of Dawson, Forty-mile, and Eagle. Thus the 1880's can be considered the beginning of white contact for the natives of the upper Tanana.

Before the coming of white fur traders to the region there was some trade with the Russian redoubts on the coast. Although the bulk of this trade was conducted through intermediaries, some was direct (McKenna 1959: 127-9; 1965: 25; McClellan 1964: 5-6). Even earlier, a few items of European manufacture reached the area from Siberia by way of a long chain of Eskimo and Indian middlemen, the so-called 'Chukchi trade' which Zagoskin investigated in 1843 (Zagoskin 1935: 205; McKenna 1965: 25). While I doubt that this early trade had any appreciable effect upon the traditional subsistence pattern, it may well have reinforced status distinctions based on wealth and thus strengthened the positions of the traditional band leaders.

Once trading posts were established in the territory, however, the developing fur trade brought inevitable changes in the material culture of the Indians and, more important, profoundly affected their subsistence pattern, round of seasonal activities, social organization, and demography. Semipermanent villages grew up in the neighbourhood of the trading posts. With the introduction of the dog team and its growing use in fur trapping, the demand for dried fish, an easily transportable dog food, increased as did the market for furs. The natives' economic life centred more and more around the individualistic activities of the nuclear family and superseded the earlier collective activities of the local band with its network of kinship responsibilities and perquisites. Hosley (1966) has noted similar changes for the Telida-McGrath Indians as has Slobodin (1962: 80-1) for the Peel River Kutchin.

The traditional way of life was further altered by a number of other factors of cultural and demographic change, listed below in chronological order of

their appearance: (1) River steamboats, utilizing native pilots and crews, brought great geographic mobility to these particular Indians, some of whom married and took up residence in villages far removed from their birthplaces, moves which resulted in difficulties in equating sib memberships. (2) Establishment of missions, and particularly mission schools, at Tanana (1887), Nenana (1907), Chena (1908), Salcha (1909), Tanana Crossing (1912), and Minto (1929) (Rowe 1920: 80ff.). These missions attracted natives from various groups and bands to semipermanent settlements along the river, further confusing the traditional sib, kinship, and residence patterns. (3) The introduction of the fishwheel in the early 1900's (Brooks 1953: 435) transferred fishing activities from the clearwater streams and interior lakes to the muddy waters of the lower Tanana (Rainey 1939: 378-9). A detailed discussion of all these changes would unduly lengthen this paper. This brief outline, however, should make clear that the traditional hunting and fishing band, which I consider to be the basic social group among most Alaskan Athapaskans, was replaced in much of central Alaska by the semipermanent riverine village.

GEOGRAPHY AND ECOLOGY

The area under discussion includes the head of the White River as far down as Snag, Y.T., and from there northwesterly in a broad belt that includes the upper Fortymile River and the entire drainage system of the Tanana River to its confluence with the Yukon at the modern settlement of Tanana; a section of the middle Yukon below and above Tanana, extending approximately from modern Galena to the 'Ramparts'; then northerly again up the East Fork of the Chandalar River to its headwaters in the Brooks Range.

This wide belt of territory, extending as it does over more than six degrees of north latitude from below 62 degrees to above 68 degrees, varies considerably in its physiographic features. It includes the rugged and heavily glaciated terrain of the upper White, Chisana, and Nabesna rivers, the lake-dotted basin of the upper Tanana, and the relatively narrow valley of the middle Tanana, which broadens again below the Big Delta River into the wide, alluvial valley of the lower Tanana. All the major tributaries entering the Tanana from the west are typical glacial streams, swift and turbid with many braided channels, and all have their headwaters in the rugged Alaska Range. The uplands to the east, between the Tanana and the Yukon, are gentler and more of the piedmont type. For the most part, the streams and rivers flowing from the east are clear, and consequently are much better suited to aboriginal fishing techniques. The middle Yukon likewise has its distinctive features, a mighty river flowing steadily through the piedmont which rises steeply from each bank and rolls on and on until it meets the mountain ranges to the north and south. While the river itself is extremely turbid, most of the tributary streams on both sides are clear. Proceeding eastward up the Yukon and omitting the lower end of the Yukon Flats, formerly the home of the now extinct Birch Creek Kutchin and the nearly extinct Yukon Flats Kutchin, the area considered here extends northward up the East Fork of the Chandalar. Although much farther north, this region has a physiography quite similar to that of the upper and middle reaches of the Tanana, viz., headwaters in the jagged peaks of the Brooks Range, a broad lake-dotted basin, followed

by a narrow valley cut through the piedmont. Unlike the Tanana the Chandalar water is clear, but this difference is unimportant since salmon do not ascend the East Fork.

This vast belt also shows some ecological variations particularly in regard to fish resources. Salmon, of course, ascend the Yukon and the lower Tanana. In the latter valley above the mouth of the Goodpaster River they cease to have any importance as a food source. Furthermore, before the advent of the fishwheel in the early 1900's, the large, turbid waters of the Yukon and particularly the Tanana did not lend themselves to the taking of salmon by native techniques. Taking salmon by means of the native fishweirs and fish spears was, perforce, confined to the clearwater tributaries of these large, muddy rivers. Whitefish, the other important food fish of the Alaskan interior, are found throughout the area I have delineated. These were taken in weirs and fish traps, generally located on clearwater streams near their outlets from lakes.

Caribou, which formerly constituted the most important single food source, are found throughout the area. Moose are largely confined to the broad river valleys and lower altitudes where browse is plentiful. They are not found on the mountainous headwaters of either the upper Tanana or the East Fork of the Chandalar. Indeed my Chandalar Kutchin informants maintained that moose had only appeared in their territory within the past two generations (McKenna 1965: 18). Although Hosley (1966) states that the moose is also a recent arrival in the upper Kuskokwim area, Lutz, a forest ecologist, doubts that the moose is recent in any part of Alaska (Lutz 1960: 14-16, 19). Be that as it may, I agree with Hosley that firearms and the more individualistic hunting practices resulting from the fur trade have greatly increased the importance of the moose in the native economy. The third big game animal of importance is the mountain sheep, which furnished skins for winter clothes as well as meat. It is restricted, however, to the higher mountains, and its habits do not easily lend themselves to the collective hunting practices favoured by the aboriginal hunters. Finally bear, both black and grizzly, are found throughout the area and in earlier times were more important as a food resource than they are today. Of the smaller animals found throughout the area, the beaver, rabbit, ground squirrel, and migratory waterfowl in season were also important food resources. Vegetable food was of little importance, although roots, particularly *hedysarum*, and berries were eaten in season.

In spite of the ecological variations, it should be emphasized that throughout the entire area the basic subsistence activity was hunting primarily for caribou and to a lesser extent for moose. Fish, both whitefish and salmon, were important food sources, particularly in the early summer, but they were definitely secondary to caribou. This dependence on caribou hunting was most marked among the Chandalar Kutchin (McKenna 1965: 28) and the groups along the middle and upper Tanana (McKenna 1959: 32), and to a slightly less extent it was also true of the Athapaskans along the lower Tanana and the Koyukons of the middle Yukon (McKenna 1964a). I think that the post-contact shift of these latter groups to semipermanent settlements along these two large rivers has tended to obscure the true nature of their

aboriginal economic base. This emphasis on hunting rather than on fishing apparently was also evident in neighbouring Athapaskan groups, viz., Southern Tutchone (McClellan 1964: 8, 9, note 5), Peel River Kutchin (Slobodin 1962: 1, 5, 10), and the Telida-McGrath Indians of the upper Kuskokwim (Hosley 1966). I suspect that in pre-contact times a similar condition also prevailed among the Koyukon of the Koyukuk River as well as among the Han and the Northern Tutchone of the upper Yukon, but full ethnographic data are lacking for these groups. In any event the ecological assumptions of both Steward (1955: 147-8, 173-5) and Service (1962: 87, note 9) underlying their theories of the social organization of the northwestern Athapaskans are not in accord with the facts as far as much of the Alaskan interior is concerned. The prevailing image of the culture of this area as being largely riverine and dependent on salmon apparently goes back to Osgood's distinction in 1936 between the Pacific Drainage and the Arctic Drainage cultures (Osgood 1936: 21). At that time, little ethnographic information was available for most of interior Alaska. By Osgood's own account the Crow River Kutchin depended as much upon hunting as upon fishing (Osgood 1936a: 31). Salmon was evidently the basic food source for most of the Tanaina, but one group, that of Upper Inlet, depended equally on the caribou (Osgood 1937: 27). Admittedly the Yukon River Ingalik, among whom Osgood (1940: 448) began work in 1934, were a riverine, fishing people, but a picture of the Alaskan Athapaskan subsistence pattern, based largely on the two most westerly groups, the Tanaina and the Ingalik, cannot help but be somewhat skewed.

GROUPINGS

Osgood, whose pioneering work brought the first order out of the previous chaos surrounding the distribution of Northern Athapaskan groups, makes this trenchant observation:

The seemingly simple problem of setting down 'tribal' names results in complications over what groups among the Athapaskans should be regarded as tribes. The Athapaskans do not consider themselves as composing neat political or cultural units. Experience in the field shows that, at least in the present state of cultural disintegration, informants generally align their village with the nearest surrounding villages and exclude those beyond a certain range, thus forming units which overlap if depicted graphically. (Osgood 1936: 3)

So far as my sample area is concerned, I agree wholeheartedly with the description of Northern Athapaskan culture as consisting not of a series of discrete cultural blocks but rather as something of a cultural continuum carried by a series of interlocking local bands whose microcultures differ in only minor details from those of their immediate neighbours. Certainly this was what I understood from my anthropological journey over the length of the Tanana Valley in 1962. Eventually, of course, these small differences build up into more significant ones, viz., those between the cultures of the middle Yukon and the Chisana-Nabesna basins, and, as is the case in this sample, cumulative process accelerates when there are also differences in the ecological setting. However, these distinctions are only apparent when comparing cultures widely separated from each other. When one moves from local group to local group, the cultural continuum is readily apparent. It is the similarities that are impressive, not the differences.

What I have said for culture in general is even more true of language. Since I am not a linguist, I have relied on informants' statements about differences in the dialects of the various local groups. Invariably the answer was the same. The neighbouring Indians spoke "just the same as us, only just a little bit different," and the next group beyond were said to speak a little more differently, and so on up and down the valley. When I moved my observation point to the next group, this picture of dialects, varying with distance, was repeated, but now the frame of reference was changed. Although in some instances the change in local dialect seemed to be a little more marked, viz., the Goodpaster dialect, the essential picture remained the same. So far as the native speakers were concerned, this linguistic continuum continued unbroken from the upper Tanana dialects to those of the Koyukon, as the latter is spoken at Tanana Village. I also have the impression that this same interlocking of dialects continued up the Yukon into Kutchin territory. Undoubtedly, from a strictly linguistic point of view, some of these dialectical differences are more important than others. Indeed to my untrained ear there seemed to be a noticeable difference in the Upper Tanana and Southern Tutchone spoken by an informant at Snag. Koyukon informants indicated that there was a difference between their language and that of the Ingalik, although they could not agree on where along the Yukon River this change occurred. Fortunately both Krauss and Hoijer are working on the whole problem of Northern Athapaskan language, Hoijer with the assistance of tape recordings of the vocabularies from ten different local groups along the length of the Tanana Valley which I made in the field in 1962. When their conclusions are published, firmer ground for grouping the Alaskan Athapaskans linguistically will exist.

The foregoing should explain why, like McClellan (1964: 6), I am increasingly uncertain about the large 'tribal' and(or) linguistic divisions of the Northern Athapaskans outlined by Osgood (1936), useful as these have been to many Athapaskanists including myself. They are certainly not 'tribal' units, as Osgood himself has taken pains to point out, and in many instances they do not appear to be distinct cultural units. If they are linguistic units, some of the lines will have to be redrawn. Indeed, if language is to be the criterion, I have already suggested that all the natives of the Tanana Valley should be classed together with the exception of those at the extreme lower end. These should be grouped with the Koyukon of the middle Yukon, whose territory should be extended up the Yukon River at least as far as the 'Ramparts' (McKenna 1964a). Finally, if purely ecological criteria are used, the result is either a few large divisions, conceivably as few as two or three, or a great many smaller units, depending on the precision and variety of the criteria used. In most of my studies, the Indians themselves were blissfully ignorant of such large groupings. Instead they thought of themselves in terms of small local bands, which, although not closed, were both social and geographic units. Occasionally several of these bands were sufficiently interlocked through marriage and common interest for them to also consider themselves as parts of a larger unit. This seems to have been the case among the several local bands that made up the Upper Tanana, the Healy River – Lake Mansfield group, the Chena–Salcha, and the Chandal Kutchin.

The totality of the Kutchin groups may constitute a special case, for there apparently was some feeling of belonging to an even larger whole (Osgood 1936: 5; 1936a: 13; McKennan 1965: 15). Perhaps this is because they have come together so much over a long period of years at common trading posts at Fort Yukon, Rampart House, and LaPierre's House. I am beginning to suspect, however, that the distinctiveness would not be so marked if more ethnographic information about their immediate neighbours, the Koyukon of both the upper Koyukuk and the Yukon above Tanana, and the Han were available.

If such traditional approaches to Northern Athapaskan grouping as 'tribe' and language are eliminated, the question remains: Is there any basic social unit beyond the nuclear family that has both a relatively permanent membership and a roughly definable territory? The wide range of social groupings for the Peel River Kutchin described by Slobodin (1962: 73-4) raises the question of whether there was any single basic unit, and McClellan (1964: 10) considers the Northern Athapaskan 'band' to be only an impermanent group which rarely functioned as a unit. Nevertheless, I believe that in the area under discussion a true social unit did exist in the form of the 'local band.' I am indebted for this particular term to June Helm's (1965) helpful discussion of socioterritorial units among the Arctic Drainage Déné. However, because of the practice of unilineal descent, the structure of the Alaskan band differed somewhat from Helm's model for the Arctic drainage. Although it possessed greater geographic mobility and persisted over a longer period of time, it was characterized by essentially the same feeling of 'group identity' and 'kin-closeness' growing as much out of 'cohesive localization' as the 'kin-connectedness' that Helm describes for the Mackenzie River area (Helm 1965: 375-6).

The primary ecological basis for these Alaskan bands was the dependence of native technology on local geography. The most important technological device was the caribou fence, either in the form of a single long fence with periodic interstices set with snares, or two long fences converging to form a central pound or corral (McKennan 1959: 48; 1965: 31). In the Tanana area these structures were generally built near the edge of timber in the hills to the east of the river toward the Yukon. Although particularly important in the early fall and late spring when the large herds of caribou were making their annual migrations, the fences were used at other times as well. Remains of old fences, stretching for five or six miles, are still to be found in the Ketchumstuk area and at the head of the Healy River; and a local informant has described to me a fence 12 miles in length near the headwaters of the Tolovana. There are recorded accounts of caribou fences extending more than 30 miles along the hills between the Tanana and Yukon rivers (Allen 1887: 138; Bales 1904: 264). Both the construction and the operation of such fences required the collective efforts of many men, women, and children. Because of the large investment of time and labour involved, these devices were used over a period of years and thus served to tie the band to a certain locality.

The fish weir also required considerable collective effort, both to build and to use. Weirs used for whitefish were located near the Tanana River on clearwater streams. Since most of these streams were small, these weirs

did not require so much effort in their construction, and a single family, or two or three related families, could build and operate them. Since weirs were generally grouped near the outlets of lakes, the band was able to maintain its group unity and identity. In a few cases, however, during the summer the caribou hunting band did split into smaller fishing bands located on contiguous small streams.

The families of the Lower Nabesna band were living together at such fish weirs along Moose Creek when I visited them there in June 1930, and Allen found similar fishing communities at Last Tetlin, Tetlin, and Lake Mansfield when he explored the Tanana in June 1885 (Allen 1887: 75-80). In regions without salmon, such as the upper Tanana and the East Fork of the Chandalar, the fishing season was limited to June and early July. Farther down the Tanana Valley in the large tributary rivers such as the Goodpaster, Salcha, Chena, and Tolovana, salmon were caught throughout the summer in a special type of salmon weir. These salmon weirs were much larger than whitefish weirs, and a single weir built across a large river such as the Salcha contained many interstices with individual fish traps, each of which was operated by a family belonging to the band. Like the caribou fences, such structures required the cooperative effort of many people. This seasonal alternation between fishing and hunting camps was still practised at the turn of the century by the Koyukon along the Yukon River. Father Jetté, who knew these people well, writes of the Kokrines group:

But, to the difference of the Kayar natives [the Ingalik], they can hardly be said to have become residents of the Yukon villages, for they still spend the greater part of their time in the Novitno region [up the Nowitna River]. . . . They come to the Yukon for the fishing season, in the earlier part of June, and return to their camps in August or September. They come out again at the beginning of December, to hold their great annual feast, at mid-winter, in one or other of the Yukon villages, and leave again for the Novitno at the close of January. Thus they spend about five months on the Yukon and the remaining seven in their Novitno camps . . . (Jetté MS.). [The identifications in brackets are my own.]

Although the caribou fence was a collective enterprise, in many cases it was regarded as the property of a single individual. The ownership of such an important subsistence item must have strengthened the identification of wealth with power and prestige that characterized the position of chief throughout interior Alaska. The position was achieved rather than inherited, although my field notes show a few specific cases where a chief was succeeded by his son or by his sister's son. Just as often, however, the chief was a man who had married into the local band. Frequently the chief was also a shaman, and this possession of supernatural power further enhanced his prestige, authority, and wealth. Chiefs were not only skilful hunters and charismatic leaders, but 'rich' men as well, and this identification of wealth with power is a theme that constantly recurs in the native myths and legends. Because of his wealth, the chief, assisted by his kinsmen, could give impressive 'potlatches' and in this manner further increase his prestige. Potlatches were simply elaborate feasts and gift-giving ceremonies in commemoration of a dead relative (McKenna 1959: 134). This ceremony was common to all the groups throughout the area except the Chandalar Kutchin. Without

question the fur trade strengthened this custom, but it appears too deeply rooted in the culture to be of recent origin.

The local bands varied considerably in size. Although exact figures are now impossible to obtain, the population of these bands seems to have ranged from about 20 to 75. The following table, based on my personal count of the composition of the five Upper Tanana bands in 1929-30, may serve as a starting point for a brief discussion of band size:

TABLE 1
Upper Tanana Bands

	Men	Women	Children	Total
Upper Chisana - Upper Nabesna	6	5	5	16
Last Tetlin	5	6	7	18
Tetlin	9	10	15	34
Lower Nabesna	16	16	27	59
Scottie Creek	6	7	12	25
Totals	42	44	66	152

In June 1885, when Allen visited this area, he found four dwellings and 34 men, 28 women, and 18 children at Last Tetlin, which he calls "Nandell's," and two dwellings with 6 men, 4 women, and 7 children at nearby Tetlin (Allen 1887: 75-7). His total of 97 persons for the two contiguous communities is considerably higher than my total of 52 for the same two groups some forty-five years later, and since Last Tetlin contained only four dwellings at the time of Allen's visit I suspect that its population may have included some visitors from nearby bands. There was also a shift of population from Last Tetlin to Tetlin in the interval since Allen's visit (McKenna 1959: 18-19). When Lowe (1899: 370) came through this area in late August 1898 he found "six good log houses" at Last Tetlin and "four log houses" at Tetlin. He gives no population figures, but he does note that the Tetlin group under the leadership of their chief, David, was leaving the next day for the fall caribou hunt. The population of the band, which ranged over the upper Nabesna and Chisana basins, was estimated at 20 Indians in 1908 (Capps 1916: 21), a figure not far different from my count of 16 in 1929.

In June 1885, Allen (1887: 80) found at the outlet of Lake Mansfield some 52 people (28 men, 18 women, and 6 children) at a fishing camp which he called "Kheeltat's" after its chief. Rice, who visited the same area in July 1899, found 50 natives camped there. Their caribou hunting camp, located in the hills near Ketchumstuk, some fifty miles away, was uninhabited at that time (Rice 1900: 786). Griffiths (1900: 726), who visited the same fishing camp only a few weeks later on August 3, 1899, described it as a "village of about 65 inhabitants." The same Lake Mansfield - Ketchumstuk band was given a total population of 73 in the census of 1890. I have quoted the pertinent reference in full, since the account contains figures for other bands as well as general demographic information:

They live in small bands, with settlements generally away from the main river, in sheltered portions, and are distinguished only by the name of the chief of each band. The Tenan Kutchin enumerated on the river and at Nuklukayet [near the modern village of Tanana], numbered a little over 300, but a few

more were reported who could not be reached by the special agent, among them Hilltah's band consisting of 73 persons (15 male and 18 female adults and 40 children, composing 16 families), who were residing temporarily across the divide on Franklin gulch in the Forty Mile diggings. Another detached band was reported as hunting on the banks of a big lake to the westwards of White river [probably Tetlin Lake], consisting of 11 male and 11 female adults and 25 children. A third band was reported on Birch Lake (Kichutin), numbering only 16 persons in all [probably either a Salcha or a Goodpaster band]. (Greenfield 1893: 126)

The locale, of course, is the Ketchumstuk area and the "Hilltah" of the 1890 census is clearly Allen's "Kheeltat." The increase in numbers in the five-year interval results from the great increase in the number of children.

While conducting a winter survey for the Eagle-Valdez telegraph line in January 1902, Lieut. (later General) W. H. Mitchell encountered a band of 13 families who were hunting caribou on the Middle Fork of the Fortymile River under the leadership of their chief, Joseph (Mitchell 1961: 68). According to my informants this was the same band that had a summer fishing camp at Healy Lake and a caribou fence near the present Joseph airstrip.

Ethnohistoric data on band size are lacking for the middle Tanana Valley, and only a few accounts are available for the lower reaches of the river. Allen (1887: 86) in late June 1885 met a group of Indians led by their chief, Ivan, en route up the Tanana River to their summer salmon fishing camp. The band contained 75 persons, including 35 men, 20 women, and 20 children. Wickersham (1938: 222), on May 19, 1903, encountered approximately 50 natives en route down the Kantishna River to the Tanana from their spring hunting camp. These possibly were some of the same band met by Allen.

Unfortunately no good information is available regarding band size among the Koyukon of the middle Yukon River, and the ethnohistoric sources give population figures for only those natives temporarily congregated around the trading posts. Information about band size among the Chandalar Kutchin is more complete. Murray (1910: 83) writes that in 1847 the Chandalar Kutchin included 40 "hunters." When I visited them in 1933 the population of the three Chandalar bands was as follows:

TABLE 2
Chandalar Kutchin Bands

	Men	Women	Children	Total
Arctic Village band	10	8	18	36
Christian's band	6	5	14	25
Old Robert's band (Venetie)	17	14	32	63
Totals	33	27	64	124

In addition, a number of the Chandalar Kutchin had taken up residence along the Yukon River. Although the movement to Chandalar Village did not take place until the latter part of the 19th century, I am of the opinion that the Chandalar Kutchin have long been divided into at least two bands, and sometimes three. My genealogies indicate that most of my informants were born in the territory of either the present Arctic Village band or Christian's band.

In February 1867 the pioneer missionary, Robert McDonald, visited a winter camp of the Chandalar Kutchin in the "Siffleux Mountains" (probably the Wind River – Smoke Creek area) which contained 65 Indians (R. McDonald: MS.). In November 1873 his brother missionary, Kenneth McDonald, visited a camp in this same general locality, consisting of five lodges. A second camp of four lodges was located a day's journey farther west (K. McDonald: MS.). The following year Kenneth McDonald visited a Chandalar Kutchin camp in the Arctic Village area. His journal entry for November 30, 1874, reads as follows:

Started early and at sunset reached the Gens du Large. . . . They are staying at their barrière [caribou fence] along the Big Lake [Old John Lake near Arctic Village]. There are six lodges there. (K. McDonald: MS.).

The typical Chandalar Kutchin winter lodge normally contained two families; thus these winter hunting camps comprised 8 to 12 families, numbering, perhaps, 35 to 65 people.

Although the foregoing data are tantalizingly incomplete, I think they do permit a few generalizations for the central Alaska area: (1) the summer fishing band and the winter hunting band were composed of essentially the same people and therefore should be considered a true social unit, the local band. (2) The caribou fence and the fish weir, often located a distance of fifty or more miles from each other, served as nucleating centres for the members of this local band. More individualistic hunting techniques, including the hunting of mountain sheep, and later fur trapping, took them much farther afield. In some instances the seasonal round of subsistence activities took a band over an area as large as 50 by 100 miles. This total area covered by the members of the local band constituted its territory. (3) The local band was small in number, containing from 20 to 75 persons, a figure considerably smaller than the composite hunting band of several hundred people that Steward (1955: 147) ascribes to the Athapaskans of the Mackenzie drainage.

The size of a particular local band undoubtedly varied greatly over long periods of time. In such small populations the effect of local famine and wars could be catastrophic, forcing the few survivors to attach themselves to more viable bands. This process can be seen in an accelerated form in the band mergings that resulted from the introduction of European diseases. It may also explain why otherwise excellent informants have such obvious difficulty unravelling the tangle of origin and migration legends that pervade the sib structure.

In emphasizing the social reality of the local band, I do not mean to deny the existence of other types of social groupings, both smaller and larger. Not all hunting or fishing was done on a collective basis; for example, winter hunting on snowshoes, particularly for moose, was often done by very small groups of hunters, or even by individuals. Frequently the small hunting camp consisted of a single commensal unit of two families living in the typical two-family dwelling used throughout the area (McKenna 1959: 75). Usually these two families were related and included either a man and his son-in-law or two brothers-in-law who were also cross cousins. Small scale war was often conducted on a sib rather than a band basis. Trading parties varied greatly in size and composition, depending on the nature of the trade and

the distance involved. With the exception of the two-family commensal unit, all such groupings were temporary.

One other important grouping remains to be mentioned, the cluster of several contiguous local bands. In the literature of the Subarctic this type of group is often called a 'tribe' for want of a better name. Such a term connotes a level of political organization that was markedly lacking among the Northern Athapaskans (McKenna 1965: 14-15). Helm (1965: 375-6) has proposed the term 'regional band' for such a group among the Arctic Drainage Déné. Both the term and the concept are applicable to the area I am discussing, particularly to the two groups I know best: the Upper Tanana, consisting of five local bands, and the Chandalar Kutchin, consisting of three. As for the intervening area, my field notes indicate that at least three other such regional bands inhabited the remainder of the Tanana Valley, and I suspect that similar groupings existed among the Koyukon of the Yukon River, although information is lacking for these Indians. In central Alaska the regional band was a territorial, cultural, and social unit. Its territory was the total region exploited by the constituent bands. Its culture included a distinctive dialect, although this distinctiveness was minimal, and other, also minor, cultural peculiarities. Socially, individual members felt that they were part of a larger unit, although they came together as a whole only infrequently and briefly. Warfare, as distinguished from vendetta, is one such example (McKenna 1959: 95-6). Potlatches are another. Finally, and most important, the local bands were bound together by a network of intermarriages. An analysis of my data from both the Upper Tanana and the Chandalar Kutchin indicates that at the local band level spouses came more frequently from outside the band than from within it; indeed in the case of the smaller bands there was no alternative. At the regional band level, however, marriages were predominantly endogamous (McKenna 1959: 118; 1964: 44; 1965: 19). The regional band, then, although far from a closed unit, tended to be an intermarrying and kin-connected group, and this in turn reinforced the feeling of unity and preserved minor cultural distinctions that might develop.

In my opinion the native population of the entire area was always sparse. By the turn of the century the dislocations and social demoralization accompanying the Gold Rush period together with the disastrous effects of diseases of white origin, particularly tuberculosis, further diminished the Indian population. Allen (1887: 137), who explored the Tanana River from Lake Tetlin to Tanana in 1885, estimated the total native population at 550 to 600. He travelled by water and saw only seven Indians between Lake Mansfield and his meeting with Ivan's band, somewhere near the mouth of the Cosna River. However, at Nuklukayet he did have the opportunity to talk with traders from the middle and upper Yukon who dealt with the Indians from the entire length of the valley. Similarly, Brooks (1900: 493), who travelled the length of the river by boat in August 1898, estimated the total population at less than 400. I think that Allen's figure is more accurate. This estimate of a population of 550-600 for an area of approximately 45,000 square miles is quite similar to McClellan's (1964: 7) reckoning that in the late 19th century there were probably no more than 250 Southern Tutchone distributed over an area of some 22,000 square miles. As I have already

indicated, the Chandalar Kutchin population seems to have remained at a reasonably steady figure of approximately 150 during the 19th century. I suspect that the same was true of the Yukon Koyukon, although comparable figures are lacking.

Legends of the epidemic diseases of white origin, which took a heavy toll during the early contact period, are deeply etched in the folk history of all these groups. The smallpox epidemic of 1838-9, which ravaged the natives of the lower Yukon (Zagoskin 1935: 340), may have reached the area under discussion. Scarlet fever, brought to Fort Yukon from the Mackenzie in 1865 (Kirkby: MS.), killed many Kutchin and spread up and down the Yukon in epidemic form during the following two years (R. McDonald: MS.). Another scarlet fever epidemic was introduced into the Tutchone area by the Chilkat in 1851 (Dawson 1888: 138*b*). One or more of these epidemics may well have spread to the Tanana. However, I believe that the effects were temporary and were compensated for by the more secure existence resulting from the introduction of modern weapons, auxiliary food supplies, and the lessening of warfare that accompanied the fur trade (McKenna 1959: 19; 1965: 21). The legendary history of the groups in question is filled with stories of famines and wars that all but destroyed entire bands. These must always have had devastating effects on a sparse population in pre-contact days, as Helm (1965: 382) has observed. So far as the Alaskan interior is concerned, I can find little support for Service's (1962: 88) thesis that the composite band of the Northern Athapaskans is the result of the catastrophic effects of white contact.

SOCIAL ORGANIZATION

Some insight into the social structure of the local band may be gleaned from a brief description of the small group at the head of the Nabesna River with whom I spent the winter of 1929. This band consisted of 16 persons (6 men, 5 women, 5 children), and constituted four households. All were related to 'Old Mama,' an ancient widow, who lived alternately in the households of her three middle-aged offspring. The four households in terms of their relationship to 'Old Mama' were as follows: (1) So, SoWi, SoSo (young unmarried adult); (2) So, SoWi, 2 SoCh; (3) Da, DaHu, DaSo (young unmarried adult), DaSo (adolescent); (4) BrSo, BrSoWi (who was also DaDa), 2 BrSoCh. All the daughter's children, incidentally, were by a former husband, now deceased. In addition to these consanguineal and affinal connections the individuals of the bands were further related through their memberships in *matrisibs*. The structure of the other local bands of both the Upper Tanana and Chandalar Kutchin (McKenna 1965: 19) was essentially the same, except that the others were larger and contained two or more family lines, or 'kindreds,' of the type just described for the Upper Nabesna. Although I have very little information regarding the structure of the various local bands in the Tanana Valley below Healy River or for the Koyukon, I strongly suspect that it was similar.

Social organization throughout the entire area was characterized by the presence of *matrisibs*. These *sibs* were exogamous, but this is not always the case today, and in some localities marriages now take place between members of the same *sib*. *Sib* affiliations also played an important part in

native warfare and in potlatches. Particular sibs were not localized but were common to large areas, although some apparently similar sibs appear under different names in bands far distant from each other. When spouses came from outside the regional band, such sib differences injected an element of confusion into the local sib structure. Although the precise number of sibs varied from band to band, the basic number apparently was three with the third sib always enjoying an intermediate and somewhat amorphous position, "almost a cousin to us all," as informants from various groups frequently phrased it. There is some suggestion that these three sibs varied both in prestige and in physical complexion, but informants disagreed on these details, even when they were members of the same band (McKenna 1965: 60-1). Informants from some bands disclaimed any knowledge of such differences.

There are a number of myths surrounding the origin of these sibs, but more often they are explained as the result of migrations of new people into the band's territory. After hearing essentially similar explanations from informants in widely separated areas, it appears that these migration legends have a basis in fact. In bands like those of the middle and upper Tanana, containing representatives of several sibs, an attempt was made to class several sibs together. This seems to be an endeavour to fit immigrants from more distant bands into the existing sib structure by equating the sib of the newcomer with a local sib. Such sib groupings resemble a moiety system, but always in the background there is that third sib, "almost a cousin to us all." For this reason I originally termed the sib organization of the Upper Tanana a phratry system (McKenna 1959: 123). Only among the Upper Tanana did I find the natives consistently using the terms 'Crow' and 'Wolf' for these sib groupings, and this was avowedly an attempt to equate their sib organization with that of their Southern Tutchone neighbours (McKenna 1959: 124).

The foregoing outline probably contributes as much confusion as it does clarification to any real understanding of the sib structure of the area. Some of this confusion, I am sure, also exists in the minds of my many informants. Nevertheless, sibs were, and to a certain extent still are, a matter of real concern to the Indians. Older informants readily discussed the matter, and I was told that the long orations customarily delivered by the old men at funerals and potlatches were for the purpose of explaining the sib memberships and kin relationships of the dead to the living.

The three-sib system, most clearly apparent among the Chandalar Kutchin, Yukon Koyukon, and lower Tanana bands, becomes less clear further up the Tanana Valley. Balikci (1963: 22-4), who worked at Old Crow in the summer of 1961, explains this third, or middle, sib as simply a convenient way of classifying the descendants of endogamous marriages. This explanation is attractive and simple. Indeed, I got the same explanation from some of my Chandalar Kutchin informants, but others specifically denied this (McKenna 1965: 61). I still regard this as simply a rationalization to explain a system no longer fully operative in 1933, and hence it was unclear in the minds of some of the natives. A century ago Kirkby (1865: 418), Jones (1867: 326), and Hardisty (1867: 315) all found a three-sib system among the Kutchin, and later both Osgood (1936a: 107, 122, 128) and Slobodin (1962: 45-6) described the same system for the Crow River and Peel River Kutchin. None of these writers mention any such mechanism

like that outlined by Balikci. Jetté (1906: 402), who lived for many years with the Yukon Koyukon and knew them well, found them divided into the same three sibs outlined for me in 1943 and 1962 by my Koyukon informants. Informants from lower Tanana bands, where the three-sib system is equally clear cut, likewise had no such endogamous explanation for this third sib. Instead, they too said that the original members of this third sib had come to their territory by a different migration route. As Eggan (1955: 542, note 160) has pointed out, strictly matrilineal cross-cousin marriage requires three or more intermarrying units in order to operate properly. Although many of these central Athapaskan groups did practise cross-cousin marriage, my data indicate that these marriages were bilateral rather than unilateral. In my opinion a satisfactory explanation for this widespread tripartite system is still to be discovered.

Students of social organization have been prone to attribute the unilineality of the northwestern Athapaskans to a riverine existence resulting from a dependence on salmon, or to diffusion from the northwest coast, or to a combination of both factors. As I have pointed out earlier in this article, the first explanation is not in accord with the ecological facts so far as the Alaskan interior is concerned, and the second explanation impresses me as being too simplistic. Furthermore, the latter does not explain the tripartite system which is found in its most clear-cut form among those Alaskan Athapaskans who are farthest removed from the northwest coast. The more I have worked with these people the more I have come to feel that in this area matriliney is a very old practice (McKenna 1959: 126-7). I gather that McClellan (1964: 8) feels much the same. Basing his conclusions on comparative data, Murdock (1955: 86) has suggested that the ancestors of the Nadené peoples brought remnants of an Old World matrilineal organization with them and that this was subsequently lost by the Athapaskan tribes that migrated to the Mackenzie. Chang (1962: 36), in the role of the 'Devil's Advocate,' has carried this hypothesis one step further, and suggested that the Eskimo, too, may once have had a unilineal organization. Helm (1965: 382) questions such speculations in her tightly reasoned argument that because of its "multiple kinship avenues to new affiliations" bilaterality possesses a survival value under the conditions of sparse and unstable population that prevailed in the Subarctic. Although Helm's hypothesis is convincing, it should be kept in mind that even the small composite bands of central Alaska that I have described possessed several kinship avenues to new affiliations, some of which stemmed from their very unilineality. However, I must agree with Heinrich (1957: 21-2) (and inferentially with Helm) that because of the sparse population which reflected the difficult ecological situation, the sib system of the Alaskan Athapaskans functioned under considerable stress and hence was constantly undergoing adaptive changes which have continued into the present contact period. Such a view does not necessarily deny Murdock's hypothesis (and mine) of the antiquity of matriliney in Athapaskan culture, and it may help to explain some of the confusion that seems to pervade the Alaskan sib system in the minds of both the Indian and the ethnologist.

My information regarding other aspects of Athapaskan social organization for this belt of central Alaska is far from complete, with the exception of the groups at the two ends, the Upper Tanana and the Chandalar Kutchin.

Ethnohistoric sources give few details of social organization, and my field work in 1962 was focused on quite a different problem. My data are fullest regarding residence patterns. Residence was matrilineal among all the Tanana groups and the Chandalar Kutchin. Following a year or more of bride service, the marriage was consummated, after which the couple lived in the dwelling of the wife's parents for another year or more. After the birth of the first child, the young couple established a separate residence in a locality of their choice; however, in most cases they remained with either the local band or the regional band. I have no information as to residence among the Yukon Koyukon, but the fact that residence practice among the adjacent Telida-McGrath group (Hosley 1966) was similar to that described for the Tanana and the Chandalar Kutchin suggests that the Yukon Koyukon were also matrilineal.

Although modern genealogies do not always confirm it, cross-cousin marriage was said to have been the preferred form in the traditional culture of the Upper Tanana (McKenna 1959: 120), Lake Mansfield - Ketchumstuk, and Healy River bands, and also was common among the Telida-McGrath group (Hosley 1966). Information on this point is lacking for the Yukon Koyukon. Cross-cousin marriages, although not favoured, also took place among the Chandalar Kutchin. The Hawaiian type kinship terminology of the latter should have worked against such a practice (McKenna 1965: 56), but this terminology may be relatively recent.

The sororate, often in the form of sororal polygyny, together with the levirate were also part of the traditional culture of both the Upper Tanana (McKenna 1959: 120) and the Chandalar Kutchin (McKenna 1965: 56). No information is available for any of the intervening groups, but the adjacent Telida-McGrath Ingalik also practised the levirate and sororate (Hosley 1966).

I have kinship terms from only the Upper Tanana (McKenna 1959: 121-3), Lake Mansfield - Ketchumstuk, Healy River, and Chandalar Kutchin (McKenna 1965: 62). All of these were bifurcate collateral in their kin terms for the parental generation, i.e., they used separate terms for Fa, FaBr, MoBr, Mo, MoSi, and FaSi. Among all these groups, with the exception of the Lake Mansfield - Ketchumstuk band, FaSi was classed with GrMo, a feature that also occurs among a few other Northern Athapaskans (Hoijer 1956: 314). In their terms for kin of Ego's own generation, all but the Chandalar Kutchin followed the Iroquois system, i.e., parallel cousins were called by the same terms used for siblings, and cross cousins were terminologically differentiated from siblings and parallel cousins but not from each other. The Chandalar Kutchin, on the other hand, followed the Hawaiian system in their terms for kin of Ego's own generation, i.e., parallel and cross cousins were called by the same terms used for brothers and sisters. The kinship terms of the Telida-McGrath bands, like those of the upper Tanana Valley, were also bifurcate collateral and Iroquoian (Hosley 1966). In the case of the Chandalar Kutchin the lack of congruence between their matrilineal descent system and their Hawaiian system of cousin terminology raises the question of which is the older. On the basis of comparative linguistic data, Hoijer (1956) in attempting to reconstruct the proto-Athapaskan kinship system demonstrates quite conclusively that this system was bifurcate colla-

teral. He is less certain regarding proto-Athapaskan cousin terminology, because these terms are lacking in his data from some groups, and the remaining groups show a much wider variety of terms than is the case for the parental generation. Hoijer (1956: 322) concludes that there is a strong indication that the parent system was Hawaiian, but data from more groups could easily change the statistic on which this conclusion is based.

The foregoing discussion reveals many gaps in our knowledge of the social organization of the central Alaskan Athapaskans. However, it does show a situation considerably at variance with that postulated by Steward and Service for northern caribou hunters. Although in most cases the bands were composite in that they contained two or more families, these families were related by both sib and affinal ties, were unilineal, and had rules of residence. This is different from Steward's postulate of a large composite hunting band consisting of many unrelated families, bilateral in descent, with no fixed residence rules (Steward 1955: 143-50, 173-7). It differs also from Service's prototype of the aboriginal hunting band which he assumes was patrilocal with reciprocal band exogamy and bifurcate merging terminology. In his theory the composite Northern Athapaskan band developed only as the result of catastrophic epidemics resulting from white contact (Service 1962: 65-83, 87-9, 108). However, this is not in accord with the situation in central Alaska where the white man is a very recent arrival while the bands apparently have long been composite with matrilineal descent, matrilocal residence, and bifurcate collateral terminology.

CONCLUSION

This discussion of the groupings and social organization of the Athapaskans of central Alaska at the time of the first white contact has of necessity been more extensive than intensive, reflecting both the wide extent of the area covered and the limitations of a short paper. However, I trust it has pointed out some features of Athapaskan culture in interior Alaska that must be taken into consideration in any general theory of the social organization of hunting peoples.

No doubt my survey has probably raised as many questions as it has answered. It certainly has indicated gaps in our knowledge which should be filled, although time is of the essence in many instances, e.g.:

1. More specific details concerning the social organization of many of the groups in the area covered by this paper are needed. I think it is still possible to obtain some of this information from older informants, an attempt I plan personally to make.
2. Good ethnographic data for both the Koyukon and the Han are woefully lacking. Although it is too late to secure complete ethnographies in the classic tradition, I am sure that some key information could still be obtained from older informants. A careful perusal of ethnohistoric sources by scholars familiar with the region and the problem could supplement this, since the time depth is so very shallow throughout the area.
3. More information regarding prehistoric and protohistoric settlements, particularly their population size and 'community pattern,' as defined

by Chang (1962: 28, 33, 37), in contrast to the archaeologist's traditional interest in 'settlement pattern' is needed. I hope to investigate some aspects of this particular problem in the Tanana Valley area, and I am sure that work now under way in the long-neglected field of Athapaskan archaeology will help.

4. There is no satisfactory answer to the question of the antiquity of matriliney among the northwestern Athapaskans. The tripartite sib system likewise requires more investigation.
5. A satisfactory explanation for many other features that set off the culture of the northwestern Athapaskans from that of the Arctic drainage is lacking. Since many of these distinctive traits are in no way related to the taking of salmon, this frequently used ecological explanation is hardly sufficient, at least for the culture of the Alaskan interior.
6. A more sophisticated understanding of the many elements common to the cultures of the northwest coast and the northwestern Athapaskans is necessary. 'Diffusion' impresses me as being too simplistic an answer.
7. Very little is really known concerning social process in interior Alaska. Although some of the basic data are forever lost, community studies of the type made by Helm (1965) in the Mackenzie River area could help fill this gap.
8. Much greater understanding of the nature of Athapaskan language and of the significance of the many regional dialects is needed. Fortunately both Krauss and Hoiyer are working on this problem, so an answer should be forthcoming.

Truly definitive information regarding many details of the social organization of the early Athapaskans is undoubtedly beyond our reach, but we should be able to arrive at a better understanding of some of its salient features by applying sharper concepts and methods to the information that is still available.

NOTES

1. Some short portions of the early part of this paper were included in somewhat different form in a paper entitled "Athapaskan Groups of Central Alaska at the Time of White Contact," which was read at the VIIIth International Congress of Anthropological and Ethnological Sciences held in Moscow, USSR, in August 1964.
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DISCUSSION

McCLELLAN: I agree with much that McKennan has written. As I pointed out in my 1964 paper (*see McKennan References*¹), I too believe that both Steward and Service have misunderstood the ecology of the Pacific drainage Athapaskans. McKennan has noted that in that paper I also discuss appropriate linguistic and social groupings for the northern Athapaskans. Since I plan a further paper on this topic, based partly on additional field-work with the Tutchone and Tagish, carried out in the summer of 1966, and partly on further consideration of material presented at the band conference, I would prefer to include most of my discussion there rather than to add much to McKennan's paper at this late date. Perhaps I should explain, however, that my remarks about the Athapaskan 'band' as being an impermanent group (*see McKennan: 100*) referred to the way in which I thought that Steward and Service were using the term.

With respect to McKennan's discussion of sibs (p. 106), I believe that the association of migration legends with sibs is characteristic of the Atna as well as of the Upper Tanana, but not of the Tutchone. Only the southernmost Tutchone groups have sibs, and they are clearly of Tlingit origin. The simple matrilineal moiety division into Crow and Wolf, which characterizes most of the Tutchone, does not have migration associations. I do not have the impression that a three-sib emphasis lurks in the background among either the Atna, Tutchone, Tagish, or Inland Tlingit. The mixed Mackenzie River – Upper Pelly River group, now centred at Ross River, also stresses moiety division. It incorporates all Mackenzie drainage natives who arrive without moiety affiliation into the Wolf moiety.

¹McClellan, Catharine, 1964. "Culture contacts in the Early Historic Period in Northwestern North America." *Arctic Anthropology*, 2 (2): 3-20.

CHARACTERISTICS OF CENTRAL ESKIMO BAND STRUCTURE

By DAVID DAMAS

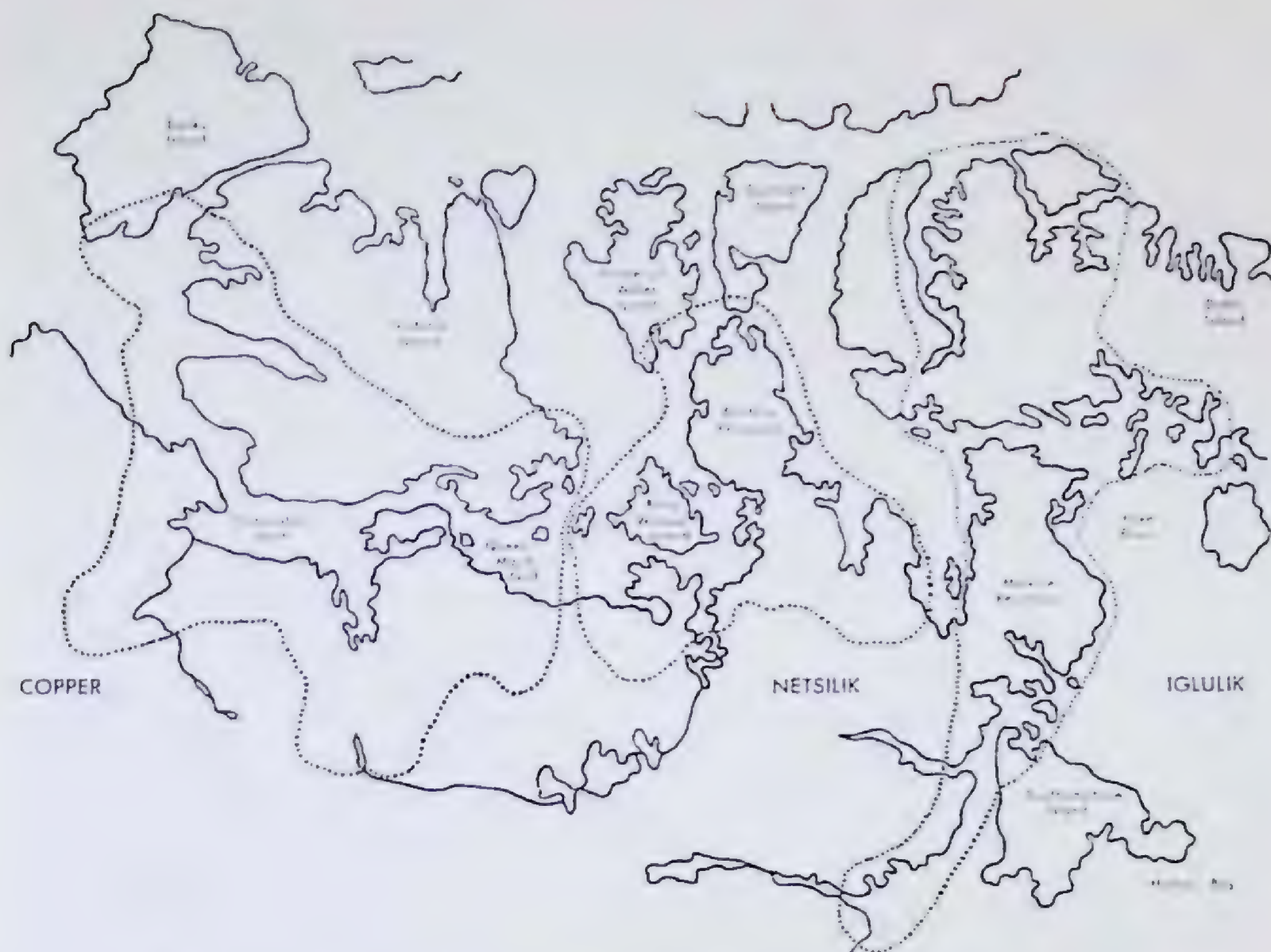
RÉSUMÉ

Les descriptions des groupes d'Esquimaux les représentent comme «réalisant un type d'intégration au niveau de la famille», «à résidence localisée dans la branche paternelle» ou «organisés en bandes composites». Le bien-fondé de ces caractérisations est examiné à la lumière des données relatives à la composition par groupes recueillies dans les trois régions habitées par les Esquimaux du Cuivre, les Netsilik et les Iglulik. Conformément à l'usage courant quand il s'agit de l'ethnographie de l'Amérique du Nord, les principales unités sociales sont définies en termes de «groupes de chasseurs», «bandes» et «tribus». En fonction des implications, par rapport à la théorie de l'organisation des bandes, de ces éléments d'investigation trouvés chez les Esquimaux, on a émis l'hypothèse que les traits suivants devaient se retrouver largement répandus dans les sociétés de peuples chasseurs:

1) les peuples chasseurs devraient normalement se constituer en groupes plus importants que celui de la famille; 2) d'étroits liens de parenté présideraient à la constitution de ces groupes; 3) la structure bilatérale serait probablement un trait dominant de toutes les bandes de chasseurs; 4) la considération exclusive du facteur parenté, dans la structure des bandes, serait de nature à fausser la tentative de les classer; 5) la bande serait probablement le plus souvent fonction d'une plus vaste unité désignée comme la tribu; 6) si l'on s'en tient aux informations recueillies chez les Esquimaux du Centre, on découvrirait, dans les bandes de chasseurs, des variations considérables affectant des caractères sociaux importants. La présentation se termine par un aperçu des textes qui ont trait aux groupements d'Esquimaux.

Eskimo groups have been characterized by Steward (1955: 24, 101) as representing the "family level of integration" and by Service (1962: 99ff) as "patrilocal bands" and "composite bands." These theorists based their generalizations on early ethnographies (Boas 1888; Stefansson 1913, 1914; Thalbitzer 1914), which, though remarkably complete in their broad coverage of cultural traits, often lacked detail about groupings. Most later accounts of the Eskimo (Willmott 1961; Vallee 1962; Honigsmann 1962, 1965) have focused on centralized communities, which have become the typical settlement in the American Arctic in recent years. The analysis of such communities has only limited utility for this Conference since its purpose is to formulate generalizations about nomadic hunting and gathering groups.

This paper treats the groups that existed in three Central Eskimo regions at a time level that can be considered aboriginal with respect to the majority of essential elements of group structure. The principal objective of this paper is to describe the group structure of the Copper Eskimo, the Netsilik Eskimo, and the Iglulik Eskimo for the period that extended into the 1920's. The analysis proceeds in terms of the 'band' as an organizing concept with reference to Eskimo society and to the theory of band organization.



MAP 1 — *Central Eskimo Tribes: Approximate Aboriginal Ranges.*

HISTORY, HABITAT, AND SEASONAL CYCLES

Recent archaeological work in the Central Arctic (Rowley 1940; Meldgaard 1960; W. E. Taylor 1964; VanStone 1962*a*) supports the view that the contact-period inhabitants of the insular and coastal regions of the Central Arctic were the biological and cultural heirs of the Thule people. Thule has usually been thought of as a culture based on whale hunting with settlement concentrated in permanent winter villages. However, W. E. Taylor's (1966) work with Thule material from the Copper Eskimo area indicates that in at least that region seal and caribou comprised the chief hunting quarries, while little evidence has been found to indicate that whaling was ever a major activity. Everywhere in the Central Arctic there appears to have been a gradual decrease in major Thule traits during a 200- or 300-year period (VanStone 1962*a*: 35). Of the three groups considered here, the Iglulik Eskimo had the greatest persistence of Thule traits (Mathiassen 1927: 63).

The three regions are characterized by long cold winters and short cool summers. Snow covers the ground from about mid-September to mid-June, and in most parts of the area the sea is free of ice for only two months of the year. Continuous expanses of landfast ice form each winter in most places, but the east coast of Boothia Peninsula and Foxe Basin have floe edges that border large expanses of open water or drifting ice.

Boas (1888: 417) has indicated the association of Central Eskimo settlement with expanses of level landfast ice that dispersed each summer. Seals are usually accessible during winter at breathing-holes, which can be made only in ice that forms during the course of one year. This factor and other features of the distribution and availability of game constituted the principal moving forces in the seasonal cycles of the three Eskimo groups dealt with here.

In the Copper Eskimo region, ringed seal, caribou, and fish were the chief game resources that influenced the movements of people during the year's cycle. In addition, polar bears were important in the winter economy of the extreme northwest (near Banks Island) and the east (Queen Maud Gulf and Albert Edward Sound).

During the winter, breathing-hole sealing was the chief occupation and generally the only practicable one, with the exception of the bear hunting previously mentioned. In the spring, the Copper Eskimo relied mainly on fish, char, trout, and tom cod. Caribou were infrequently hunted at that period because their hide was poor and the meat lean. During the summer, caribou hunting became as important an activity as fishing. In the autumn caribou were hunted intensively to secure meat for food and skin for clothing. During the late fall the Eskimos camped at coastal localities and lived on cached food while the women sewed their winter garments. In December or January a descent to the ice was made for the period of breathing-hole sealing. Muskoxen, although hunted whenever they were encountered, were not a significant source of meat and furs, except around Bathurst Inlet.

The seasonal economic cycle of the Netsilik appears to have been virtually identical to that of the Copper Eskimo. The chief activities that shaped the year's cycle were fishing, breathing-hole sealing, and caribou hunting from kayaks or at drives. Bears could be encountered almost anywhere in the area. Although muskoxen inhabited this area, they were not an important factor in the Netsilik economy.

Copper and Netsilik Eskimo practised *uuttuq* (hunting of the basking seal in spring) only occasionally, and they did not use the kayak in the sea.

Principally, subsistence in winter for the Iglulik Eskimo was dependent on breathing-hole sealing. At Iglulik Island and around Repulse Bay thin-ice, walrus hunting was also an important winter activity. Those Eskimos who lived near Depot Island and Chesterfield Inlet hunted the walrus and also relied on muskoxen during the winter. *Uuttuq* sealing was extensively practised throughout the Iglulik Eskimo region. An important summer activity was the hunting of sea mammals, principally seal and walrus, but also narwhal and beluga. During part of the summer, groups split into two segments—the younger men and the more mobile family members hunted caribou inland, and the older men remained at sea to pursue sea mammals. Two types of kayaks were used, one for caribou hunting in lakes, and another, a heavier one, for use in the sea. Fishing was not widely practised.

In summary, the Copper Eskimo and Netsilik generally devoted a greater portion of the year to fishing and caribou hunting than did the Iglulik Eskimo who placed greater emphasis on sea hunting.

These descriptions of seasonal cycles are abstracted from the accounts of early explorers by Boas (1888: 444–59) and supplemented by data from

later ethnographers (Jenness 1922: 110-44; Mathiassen 1928: 23-36; Balikci 1964: 41-3) and by my own data taken from aged informants in the three regions. Basically, they represent traditional or aboriginal cycles that probably apply to the 19th century and in some places to the early years of the 20th.

The most important sources of data for the three regions are the reports of the Fifth Thule Expedition of 1921-4 (Mathiassen 1928; Rasmussen 1930, 1931, 1932; Birket-Smith 1945). Among the data contained in these reports are remarkably accurate censuses of the regions. Working with genealogies gathered in the field, I have been able to reconstruct group composition charts for the following: The Iglulingmiut¹ of the Iglulik Island region; the Netsilik of Pelly Bay, Boothia Peninsula, and Adelaide Peninsula; the eastern groups of Copper Eskimo who hunted in Bathurst Inlet, eastern Victoria Island, and Queen Maud Gulf. These charts provide the basis for much of the discussion that follows.

CULTURE CONTACT AND GROUP STRUCTURE

The visit of the Fifth Thule Expedition was preceded in the Central Arctic by a period of contact with exploratory parties, traders, and whalers. It is therefore necessary to assess the possible effect that this contact may have had on the social groupings of the area before assuming that they represented aboriginal types of aggregations. The chief ways in which culture contact could be expected to affect group organization are changes in the seasonal pattern of aggregation and changes in population size or structure due to depopulation or immigration.

Possible changes in group dispersal and aggregation can be related to changes in technology which are expected to affect hunting practices, and correlatively, seasonal economic cycles. Indeed, the use of the rifle to hunt caribou had begun to alter the pattern of activity in the three regions (Jenness 1922: 101; Mathiassen 1928: 53ff; Rasmussen 1931: 57ff; Balikci 1964: 44ff). The practices of driving animals between rows of rocks (*inuksuit*) toward waiting hunters and of kayak hunting in lakes were abandoned in favour of less organized hunting with rifles. Caribou hunting also extended over a longer period of the yearly cycle than it did in the bow and arrow and lance era. Other basic features of the aboriginal seasonal cycles in all three regions appear to have persisted up to the time of the Fifth Thule Expedition. The Iglulik Eskimo groups continued to split their economy between caribou hunting and sealing from boats during the summer phase of the cycle. Floe-edge sealing with rifles was only beginning to affect the winter economy at Repulse Bay and Pond Inlet (Mathiassen 1928: 26). The Netsilik and Copper Eskimo had not yet seriously adopted sealing from boats in summer.

The most important factor that concerns our interest in groupings was the persistence of the large winter villages that are associated with breathing-hole sealing. Jenness (1922: 248) notes that the Copper Eskimo around Kent Peninsula stayed inland at trapping camps during the winter of 1919-20, but this circumstance appears to have been only temporary, for Rasmussen's account (1932: 12, 119) indicates the establishment of winter sealing villages for the Kent Peninsula and Bathurst Inlet people in 1923-4, though their

seasonal cycles were drastically altered a year or two later. Hoare's summary of Copper Eskimo cycles for the period 1924-6 is interesting in this regard:

At the present time the majority of the natives leave off sealing the middle of March, proceed to the Arctic mountains and there take up their stand in the passes (for intercepting caribou). The only exception in the district I visited these past two years being the Bathurst Inlet people who spent last winter in the interior hunting caribou and trapping foxes until the middle of March. Then they came out on the sea ice to obtain sealskin for boots, in May returning to land to again hunt caribou. (1927: 37)

It appears, therefore, that the duration of the sealing villages of the Copper Eskimo became shortened, but large aggregations continued to occur in connection with winter sealing for some years after the visit of the Fifth Thule Expedition.

Large winter sealing villages are also reported for the Netsilik region for the middle and late 1920's (Burwash 1931: 28, 49, 74; Balikci 1964: 48).

Around Iglulik the winter villages appear to have become somewhat more fragmented during the same period with 50 persons apparently being the normal maximum winter aggregation (RCMP 1924, 1929, 1930, 1931).

While the bulk of the population of the three areas aggregated in major winter sealing villages, there were three smaller aggregations in the Iglulingmiut region of 10, 17, and 27 individuals (Mathiassen 1928: 17-19) and one detached Netsilik group of 16 (Rasmussen 1931: 88) referred to in the census data. These smaller aggregations possibly represent the beginning of the more dispersed populations that later characterized settlement in the Central Eskimo area. However, detached winter groups may have always existed in the Iglulik Eskimo region (Parry 1824: 430; Boas 1888: 444; Mathiassen 1928: 29). The existence of smaller winter units both there and at Bellot Strait in the Netsilik region can usually be associated with regions of superior resources. In these small groups the women had to be employed to watch a sufficient number of breathing-holes to make winter sealing possible. This would appear to be a substitute method at best. The establishment of all these smaller aggregations at good large sea mammal sites indicates that stores from the summer hunts may have provided a large part of the winter food supply. It appears from informant recall data that most of these outpost camps probably were of fleeting duration, existing for several years before amalgamating with the larger villages.

The history of the members of the Bellot Strait group is interesting in that the diverse origin and loose kin ties of the members appear to validate Rasmussen's (1932: 88) contention that this camp was composed of exiles from various Netsilik regions, men who had stolen wives or committed homicide.

Since the trading posts had already been established in the Copper Eskimo area (Kent Peninsula 1920, Willmott Island 1922, and Bernard Harbor 1916), the Eskimo of this region were able to obtain rifles and other trade goods.

The first trading post was established in the Netsilik area in the autumn of 1923 just as Rasmussen was leaving the region (Rasmussen 1931: 80). The Iglulingmiut continued to make long trading trips to Repulse Bay and Pond Inlet. Although trapping had been practised for at least a few years

in all three regions, it had not yet reached the stage of long trap-lines but was restricted to the immediate vicinity of villages and caches.

Service (1962: 100ff) referred to evidence of drastic population reduction throughout the Eskimo area during this period. This decline in numbers, which he attributed to the introduction of disease, laid the basis for important changes in group structure which he felt had occurred by the time the ethnographic material dates. These changes in structure relate to the alteration of local groupings from 'patrilocal bands' to 'composite bands' (Service 1962: 107). Although the evidence for this sort of depopulation appears to be strong for the Mackenzie, southern Baffin Island, and parts of Greenland, it is not convincing for the regions discussed in this paper.

In the Iglulik Eskimo regions, Mathiassen's census for 1922 (1928: 14) compares very closely with Parry's (1924; also Manning 1943: 103) estimate for 100 years earlier with respect to total number (about 500 in each case), and ratios of males to females and of adults to children (Damas 1963: 23).

Boas (1888: 450) believed that early accounts indicated a population of 350 for "Netsilikmiut" during the 19th century, but his estimate did not include the Pelly Bay or Adelaide Peninsula groups which are also considered to be Netsilik Eskimo (Rasmussen 1931: 85-90). Taking those two groups into account, it would appear that the 19th century population of the Netsilik region was probably between 450 and 500. Although Rasmussen (1931: 84) gives a population of 259 for the 1923 census of Netsilik Eskimo, my reconstruction of the same population yields 264. The apparent decline in these figures, when compared with the estimate for the 19th century, can be explained in terms of emigration to the Repulse Bay - Chesterfield coastal areas. This movement, which began about 1900, was associated with contact with whaling fleets (Mathiassen 1928: 101-2; Manning 1943: 102). Rasmussen (1930: 84-8) gave the names of 156 Netsilik who had settled in that area and stated that "30 or 40" others had moved down from the north. This number when added to the census figures from the aboriginal territory of the Netsilik brings the total to approximately 450, which compares closely with my estimate for an earlier period.

It is probable that population fluctuations caused by famines occurred in Netsilik groups; for example, Rasmussen (1931: 134-5) lists 25 persons around Adelaide Peninsula who perished in 1919 as a result of starvation caused by adverse weather conditions. This sort of situation probably occurred occasionally in earlier periods as well. It is therefore not necessary to explain the apparent reduction in Netsilik population as a result of disease introduced from outside. The assessment of the effects of the emigration of 180 to 200 Netsilik to the south, with respect to group organization, will follow later in this paper.

There is no 19th century estimate of Copper Eskimo populations with which to compare later estimates and censuses. Stefansson (1914: 25-40) guessed that there were about 1,100 Copper Eskimo in 1911, but Jenness (1922: 42) criticized that figure, himself putting the 1914-16 population at 700-800, a number that compares well with Rasmussen's (1932: 70) total of 814 for 1923-24. There is evidence, however, for extensive depopulation around Bathurst about 1850, which may have been responsible for

peculiarities in the structure of the Bathurst Inlet group of 1923–24. Analysis of this group will be covered below.

Service's (1962: 100ff) citation of Stefansson's (1913:25) indication of larger populations for the earlier Copper Eskimo and Mathiassen's (1928: 2) similar assessment for the Iglulik region seem to be clearly based on archaeological remains and refer to Thule and earlier culture periods. They do not appear to point to declines in population resulting from disease acquired through contact.

In summarizing the possible effects of culture contacts on Central Eskimo groupings, it should be noted that despite some changes in the seasonal cycle the winter sealing village and smaller hunting bands from which they drew their members continued as the main settlement types. In addition, informant recall information indicates that hunting areas remained essentially the same (with the main exception of the relocation of Netsilik to the south) and that individual identification with one or the other of the winter aggregations continued. There appears to be no sound basis for hypothesizing large scale depopulation in the three regions. Accordingly, there should be justification for regarding the charts I have drawn of the groups for the 1921–24 period as basically aboriginal in character, though it is necessary to be alert to the possible effects of population movements in the case of the Netsilik and local depopulation in one Copper Eskimo locality.

IDENTIFICATION OF THE CENTRAL ESKIMO GROUPS AS BANDS

A number of features were common to the groups of these three regions. The most important of these was the maximum aggregation of personnel associated with winter breathing-hole sealing, although in some areas the maximum group size was maintained throughout the period from January to May. Elsewhere personnel sometimes split into adjacent and communicating villages (Mathiassen 1928: 30; Rasmussen 1931: 22). Usually at some point during the winter the Netsilik population of 450–500 (prior to 1900) converged in four main sealing villages. The Copper Eskimo, approximately 800 persons, concentrated in seven or eight such villages (Jenness 1922: 277), and the Iglulik Eskimo population of about 500 in five or six camps. Therefore, the average size of the maximum aggregation was about 100 persons. The summer hunting groups averaged about 20 persons and appear to have fluctuated between 5 and 50 members. The larger number assembled at the sites of caribou drives and fish runs. Groups of about 50 also gathered at the end of autumn when winter clothes were being sewn. A number of small hunting groups (often the unit which gathered for the sewing season) were considered together under the regional designation represented by the *miut* affix. The larger winter aggregation was also called *miut* according to a larger regional identification, though at times it appears that this later usage relates mainly to a particular winter grouping by neighbouring units. Everywhere in the area the winter aggregation comprised an important ceremonial entity. Shamanistic performances, dancing, and athletic contests played an important part in winter life. The overall leader, or *isumataq*, in the Iglulik Eskimo winter groupings gave the winter village a quasi-political aspect.

A more or less consistent picture of the defining characteristics of the 'band' emerges from the literature dealing with North American Indian hunting and gathering peoples (Honigmann 1946; Eggan 1955; Opler 1955; Steward 1955; Slobodin 1962; Rogers 1963; Helm 1964). The term 'band' has most often been associated with a group varying in size from 50 to over 300 persons. There is usually an identification by name with the general territory that is exploited, though this does not necessarily imply defence of a territory. The band generally seems to be composed of smaller segments that operate within the general area designated by the band name. Periodic meetings of these segments generally take place, at which time there are usually associated ceremonial activities. There is seldom if ever a year-round local aggregation of all the members of the band. In some cases there is a band leader or chief.

In short, the groupings of Central Eskimo, which have been described above, possess general characteristics that conform to criteria used in identifying bands in the ethnological literature. Accordingly, the remainder of the discussion will proceed in terms of bands and band structure.

ANALYSIS OF THE KILLINIRMIUT BAND CHART

Examination of the group composition chart of the Killinirmiut band of Copper Eskimo who wintered on the ice of Dease Strait can increase understanding of character of the eight bands that comprise the sample for the three regions.

With regard to size, this band had 99 members, a population comparable to the average of 89 for the eight bands. It should be noted that the average is lower than the estimate of 100 for the aboriginal period because of three aberrantly small Netsilik bands whose smaller size is caused by the emigration situation noted previously. Therefore, the Killinirmiut band can be regarded as a typical Central Eskimo winter aggregation with regard to size.

Another feature of this band is the extensive continuity of kin links. Seventy per cent of the individuals in the band form a continuous linkage through surviving primary relatives and an additional 16 per cent included if a recently deceased marital pair is considered (as shown in the chart). These percentages are fairly typical of the eight bands, some of which approach 100 per cent linkages. Only the somewhat anomalous Bathurst (Kiluhikturmiut) band shows an appreciably smaller number of links.

There are more males than females (55:43) in this band. Similar proportions are found in the other Copper Eskimo bands and in the Netsilik region as a whole, whereas in the Iglulik region the ratio of sexes is about even. This disproportion attests to the practice of female infanticide in the former two regions (Rasmussen 1931: 139-41; 1932: 70).

Although marriages between related persons seem to have been the preferred mode among the Copper Eskimo (as well as the Netsilik), there is only one case actually shown on the chart. Most of these marriages occurred between relatives of third and fourth degrees of collaterality and would not be revealed on charts like the Killinirmiut based on primary relationships. In this respect the Iglulik chart resembles the others, even though kin exogamy is preferred and was usually adhered to.

KILLINIRMIUT

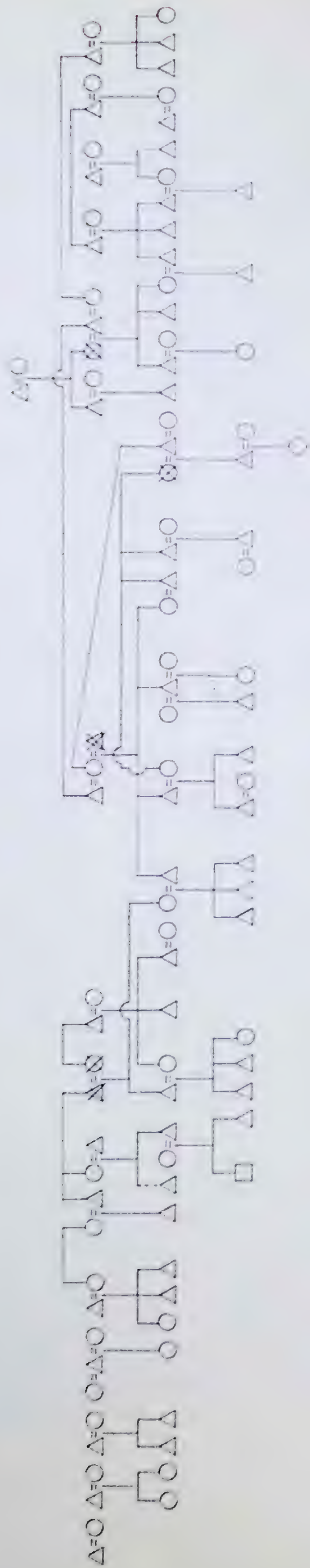


FIGURE 1 — Kinship composition of Killinirmiut band.

The two cases of polygyny and one of polyandry shown in the chart are interesting in view of the predominance of males in the band. There are three cases of polygyny and two of polyandry in the Copper Eskimo bands and the same number in the Netsilik bands that are considered here. These overall ratios do not reflect an expected polyandrous situation and may support Rasmussen's (1931: 195) assertion that polyandrous marriages were less successful than polygynous ones.

Another feature of this chart is the predominance of male oriented kin ties over female oriented kin ties. Using Helm's² procedure of band composition analysis, 51.4 per cent of the ties were male critical, 24.3 per cent female critical, and 24.3 per cent neuter. This compares with overall figures for nine bands, including the Utkuhiksalingmiut or Back River people, of 49.8 per cent male critical, 28.8 per cent female critical, and 21.8 per cent neuter. Therefore the Killinirmiut band is quite typical in these ratios of ties.

Thus it is possible to see this band chart as representative in most features that can be inferred from such documents for the eight bands in our sample. Two exceptions can, however, be noted.

The first of these is the case of the aberrantly small Netsilik bands which also displayed sex linkage ratios at odds with the Killinirmiut band and with the general averages found in all the bands. It has been noted earlier that emigration from the traditional hunting area had occurred in the period immediately preceding Rasmussen's visit in 1923. The emigrants seem to have been drawn from the Pelly Bay, Thom Bay, and Rasmussen Basin bands, which are anomalous in their small populations of 63, 46, and 64 respectively as compared to the more typical 87 of the Illuilirmiut or Adelaide Peninsula band. Although these three bands all show the same high degree of internal linkage found elsewhere, there appears to be a definite connection between small size of band and irregularity in male-female linkage percentages. One of the bands has a rather high ratio of male relevant ties, 58.9 per cent, and the other two have a preponderance of female relevant ties, 41.7 per cent and 40.0 per cent. If the totals for the four Netsilik bands are added together, however, the average is very close to the overall figures given above.

A second aberration is the low total bandwide linkage in the Kiluhikturniut or Bathurst Inlet band, where there is much kin disconnectedness. Jenness (1922: 38) noted that the individuals who comprised the 1916 aggregation of this band entered the region from wide-ranging localities, a situation compatible with both Jenness's (1922: 39) report of a probable epidemic in the region and Service's (1962: 108) general hypothesis regarding migration from surrounding regions into depopulated areas. Examination of the Kiluhikturniut band chart indicates, however, that sizeable blocks of kin had been established by 1923-24, three or four generations after the possible epidemic. Such relatively composite bands would seemingly tend to reconstitute themselves according to the pattern of band structure that characterized the Central Eskimo regions. Since, contrary to Service's statement, there were formerly no rules against band endogamy, marriages within the band tended to establish kinship linkages that would in time reform the band as a continuous or near-continuous entity similar to that of the other bands of the sample.

Thus it is possible to gain some insight into band structure by examining group composition charts. However, there is other information on social structure requiring data that cannot be inferred from the charts alone.

RESIDENCE, MARRIAGE, AND THE 'TRIBE'

It has been noted that there is an overall slant toward male relevant kin ties within the bands. In the Iglulik and Netsilik regions there is an expressed ideal for virilocal residence. In the model patrilocal band (Service 1962: 66ff) father-son and brother-brother ties would make up the bulk of connections. While there is a substantial number of such ties shown on the charts, the scope of unilateral connection is generally narrow, and there are a great many affinal connections. In other words, bilaterality is pronounced. Given the ideal of virilocal residence, a high rate of exploitation of potential ties might be expected, and it might be hypothesized that the limitation in the scope of virilocally constituted blocks could be accounted for in terms of population depletion. Examination of genealogies shows, however, that there is potential for more expansive male sibling groups to exist as co-residential blocks but that siblings are often separated spatially among the bands in the same general region. Budding of this sort is most pronounced in sibling groups where the parents are deceased, indicating that the parent-child bond has greater cohesive force than the male sibling bond. There are, to be sure, a number of instances of males who have been isolated by the deaths of consanguines. Such individuals regularly associated with their spouses' kin rather than with their own secondary consanguines who were often residing in other bands. This feature is an important factor contributing to the degree of bilaterality found in the bands of the sample.

An understanding of the distribution of sibling groups, and ultimately an assessment of their role in determining the degree of bilaterality or unilaterality in the bands is possible only if the genealogical horizon is extended to include the entire population of each of the three regions. This expansion is also necessary to gain an understanding of marriage practices. In the Netsilik and Copper Eskimo regions there is a preference for marriages among relatives. It was indicated above that only a few of these marriages were expressed in the band composition charts based on primary kin ties; however, more appear if the total genealogies of each of the major regions are examined.

In all three regions there was an expressed preference for marrying within the major region. Thus an Iglulik Eskimo generally married an Iglulik Eskimo, a Netsilik another Netsilik, a Copper Eskimo another Copper Eskimo, though this pattern was beginning to break down at the time of the Fifth Thule Expedition. Even at that time probably 90 per cent of the marriages occurred within the Netsilik, Iglulik, and Copper Eskimo groupings while earlier the percentage may have been in excess of 95.

Although there was much fluidity of personnel among the bands within the major regions, it appears that 60 to 70 per cent of the members of one winter's sealing aggregation - band assemblage would return the following winter so that a core of members remained from year to year. The larger entity, however, showed much greater stability over a longer period. Although each of the complete genealogical pools of the Iglulik, Netsilik, and Copper Eskimo showed a high degree of continuity within as well as

discreteness with regard to one another, they never comprised face-to-face groups.

Helm (1964: 5) has suggested the use of the term 'tribe' to designate such groups "who know themselves and are known as 'Dogribs,' Hares, Slaveys and are socioterritorial entities evincing the greatest duration, and with recruitment, in the sense of conscious decision to affiliate, at a minimum." Rasmussen (1925), Mathiassen (1928: 1), and Birket-Smith (1945: 15) have spoken of the Iglulik, Netsilik, and Copper Eskimo 'tribes' using, as criteria, cultural similarities within the 'tribe' and their identification as a separate group by outsiders. To these criteria, as well as probable linguistic criteria, can be added the general pervasiveness of kinship ties within the entity. The occurrence of the genealogical continuity within and discreteness without the major groupings would appear to be sufficient justification for their separation conceptually. The term 'tribe' could well be used to identify each of these clusters of bands if it is understood that the political implications often associated with that term do not apply.

INTERNAL UNITS AND THEIR INTEGRATION

In turning the analysis away from external band associations toward an examination of internal units, the most important considerations revolve around characterizing the family or household. My data on family structure in the Copper Eskimo region agree with those of Jenness (1922: 74, 84ff) for the region farther west. Typically, each nuclear family possessed its separate dwelling. When composite snowhouses were used, household composition was irregular in nature. That is, those nuclear families who shared dwellings or compounds in one encampment probably would not do so in the next (Jenness 1922: 74). Combined households appear to have been composed as often of unrelated as of related nuclear families. In some cases the heads of families who shared dwellings were spouse exchange partners.

With regard to the economic criteria of family organization, sharing did not take place automatically among fathers and sons or among brothers. Rather, each individual divided his catch of seals with a number of partners who could be either kin or non-kin, and the partners would reciprocate with a corresponding part of the seal. An aid to unifying the local settlement at any time of the year was the Copper Eskimo practice of village-wide communal eating. Generally the practice at mealtime was for the members of one snowhouse to entertain most of the other villagers in shifts, with each household taking a more or less regular turn in the cycle of entertaining.

Spouse exchange partnerships and partnerships in joking and dancing seem to have been contracted only between non-related persons. Such arrangements helped to unite band and tribal members in sentiment.

In short, Copper Eskimo family organization probably represented as close an approach as will be found to the isolated nuclear family attributed to the Eskimo in interpretive writings (Murdock 1949: 227; Service 1960: 77; 1962: 99). Social structuring within the band was based largely on partnerships which seemed for the most part to operate independently of existing kinship ties. Kinship did, however, play a role in the arrangement of marriages, in some cases of adoption, and in the hard to objectify area of sentiment.

If kinship factors were minimized as structuring mechanisms among the Copper Eskimo, they were most prominent in the Iglulik area. With regard to residential situations, dwellings generally housed at least two closely related nuclear families. In addition, individual domes occupied by people within a close kinship network were often connected by short passages. There was a definite virilocal emphasis within these establishments although there probably was at least one uxori locally attached nuclear family in the larger extended families. Each of these households was headed by a family leader, or *isumataq*, almost invariably the oldest male, who organized group efforts and regulated sharing within the extended family. The whole local community, whether it was an entire band assemblage or a smaller segment, had its *isumataq*, who was usually the leader of the largest or most productive extended family present. He regulated village-wide organization of hunts and sharing of catches, and distributed food among the leaders of the various extended families which formed indivisible sharing units. The commensural unit appears at times to have been the extended family, and often the nuclear family or even the individual.

The Iglulik bands formed tight cooperative units which exhibited close emotional attachment based on a broad-ranging network of kinship behavioral directives that outlined behaviour appropriate to a large number of each individual's band associates (Damas 1963: 46ff).

In the Netsilik region the extended family appears to have been as strongly developed as in the Iglulik region. Dwellings were usually composite with individual domes often shared by two closely related nuclear families who were in turn related closely to others within the compound (field notes and Steenhoven 1962: 63). Sharing took place automatically among members of the extended family. Each extended family comprised two commensural units: one, the men of the household, and the other the women (field notes; and Balikci 1964: 34).

As in the Iglulik region the extended family was generally led by the oldest male, called the *ihumataq* (field notes; and Steenhoven 1962: 55); however, there does not appear to have been an overall leader or band *ihumataq* responsible for food division. At times a prominent hunter may have organized hunting parties, which admitted individuals outside his own extended family, even to the extent of including the entire village.

Seal-sharing partnerships insured the distribution of food among the extended families. Ideally, partners were non-kin or distant kin born in the same year. The most important difference in seal-sharing practices between the two regions (Netsilik and Copper Eskimo) was that in the Copper Eskimo region the partnerships resulted in sharing between nuclear families, whereas in the Netsilik region the shares received from each individual's partners ultimately became the property of the extended family.

To summarize the characteristics of internal organization in each of the regions: in the Iglulik Eskimo area, kinship factors were ascendant over voluntary associations in band structure; in the Copper Eskimo area, partnerships appear to have had greater importance in unifying the bands within; and in the Netsilik area, there appears to have been a balance between two types of organizing mechanisms.

COMPOSITION OF THE HUNTING GROUPS

There appears to have been variation in the structure of the hunting groups which comprised the typical local aggregations during much of the seasonal cycle and combined in winter to form the band assemblage. In the Copper Eskimo area the structure of the summer groups was irregular. In some cases primary kin ties existed between hunting group associates, at other times spouse exchange pairings were involved, and occasionally there seems to have been neither close kin ties nor extrakinship formalized partnerships involved. The size and membership of groups fluctuated widely during the course of the most nomadic phases of the yearly cycle. The sharing of currently secured game, for example caribou, proceeded mainly along the lines of communal eating, but at times the system of seal-sharing partnerships or other voluntary sharing arrangements may have applied.

Iglulik Eskimo hunting groups were usually based on kinship ties coordinated with extended family organization. During part of the summer the younger men (usually siblings or siblings-in-law) hunted caribou inland. The older men, in pairs or groups of three, stayed at the coast hunting sea mammals from kayaks. At times, though not always, these men were closely related. The products of the inland and coast hunts were eventually shared by the entire family when the segments of that unit reunited in the fall.

Netsilik hunting groups usually comprised the extended family, normally intact throughout the caribou hunting and fishing phase of the yearly cycle. At times peripheral units attached themselves to the extended family, and often two or more extended families combined to form a hunting group. Spouse exchange pairings were also at times important to group composition. Individual nuclear families occasionally broke away to visit other kin.

SUMMARY OF CENTRAL ESKIMO BAND STRUCTURE

Each Central Eskimo band was composed of several hunting groups which coalesced for a period during each winter in connection with breathing-hole sealing and ceremonial activities. This band assemblage comprising a nearly continuous genealogical unit had a male relevant or virilocal emphasis.

Regional variation is apparent in internal structuring. The bands of the Copper Eskimo can be characterized as clusters of nuclear families held together as much by extra kinship as by kinship associations. Copper Eskimo summer groups were irregularly structured. The Iglulik Eskimo bands were generally made up of several extended families united by kinship behavioral directives and organized under the leadership of an overall band *isumataq*. The extended family maintained its importance during the summer phases, even though it was, at times, split spatially. The Netsilik bands were also composed of extended families, but in the absence of an overall band leader they were united mainly through partnerships ties.

Each band related to a larger entity designated as the tribe. The tribe comprised the marriage universe and formed a nearly continuous genealogical entity possessing greater stability in membership than the bands. These tribes were set off sharply from one another genealogically and showed a high degree of uniformity in social features within their areas.

POSSIBLE IMPLICATIONS FOR THE THEORY OF BAND ORGANIZATION

Questions can be posed regarding the applicability of the data available on Central Eskimo bands to the study of hunting and gathering bands elsewhere in the world. For the purposes of comparing this data with other material that is being presented at this conference, I will outline a few generalizations that seem warranted from my study of bands in three Eskimo regions.

(1) *Hunting peoples generally aggregate regularly in groupings larger than the family.*

Although visiting may play an important role in establishing and maintaining ties at the family level of aggregation, periodic assemblages of larger units do occur. Economic advantages (such as cooperation in breathing-hole sealing) as well as ceremonial and social motives depend upon periodic aggregations such as band assemblages.

(2) *Hunting bands are usually internally linked by extensive kinship ties.*

The character of the Eskimo bands treated here suggests that a continuous kinship linkage within hunting bands occurs. This pattern would seem to be assured if no rules of local exogamy are enforced. Indeed it may well be the case among nomadic hunting peoples that because of the flexible nature of composition and the mobile character of the band, marriages tend to be regulated according to kinship rather than to local considerations.

(3) *Bilaterality is probably a prominent feature in all hunting bands.*

The isolation of individuals as a result of the death of agnates and the small number of children who survive to adulthood, together with an imbalance in the sex of offspring from family to family, are features of population structure found in the Central Arctic and probably elsewhere in the world under the conditions of hunting or gathering life. Although unilocal residence rules or preferences may prevail in hunting societies, great difficulty is encountered in actualizing them because of limitations imposed by the population structure.

In addition to these factors, it seems likely that in societies like the Copper, Netsilik, and Iglulik Eskimo that lack the lineage principle, preference for ties with close affines prevails over associations with more distant consanguines. Both social and demographic factors appear to conspire against the development of expansive unilocality in bands.

(4) *The exclusive study of kinship factors in band structure may hinder attempts at classifying bands.*

Even though it has been pointed out that kinship bonds were extensive in the Copper and Netsilik bands, the examination of kinship factors alone seems to give a highly incomplete picture of the structure of those bands. This suggests that non-kinship features, for example partnerships, among the Central Eskimo may be of equal or greater importance than kinship features in the social structure of some hunting bands. This possibility places limitations on typologies of bands based mainly on marriage or residence practices.

(5) *The band probably relates often to a larger unit designated as the tribe.*

Generally speaking, bands comparing roughly in size to those of the Central Eskimo do not constitute optimum breeding units. Therefore the genealogical universe will be a larger grouping. This unit appears to be relatively discrete in the Central Arctic and corresponds to a dialect area and cultural subarea. This situation suggests questions regarding the discreteness of genealogical horizons elsewhere and the congruences of cultural, dialectal, and kinship universes.

(6) *The data on Central Eskimo bands suggest considerable variation in important social features of hunting bands.*

Variability in the composition of hunting groups which make up the bands, in family organization, and in marriage practices among the three regions discussed here suggests that such factors will show an even greater variation over the hunting and gathering world. It is to be expected, therefore, that problems of reducing all hunting and gathering bands to several basic types will be compounded when the full range of this variation becomes clear.

NOTE

1. In *Igluligmiut kinship and local groupings: a structural approach* (1963), I used the spelling 'Igluligmiut.' Later, when I became more familiar with the language, I established 'Iglulingmiut' as the correct spelling, as it appears in this publication.
2. This system of analysis will be dealt with in the discussions on "Methodology of Group Composition Analysis."

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APPENDIX

SURVEY OF ESKIMO GROUPINGS

In focusing the discussion on three Central Eskimo 'tribes,' there have been two advantages: working with data gathered mainly in personal field-work and treating the groups concerned in sufficient detail to make possible fruitful comparison with the material given in the other papers. It might be useful to broaden the scope of the study to include other Eskimo groups to indicate something of the range of grouping types that can occur in hunting societies.

The Utkuhiksalingmiut, or Back River people, shared a common dialect, and many features of material culture with the Netsilik (Birket-Smith 1928, 1945; Rasmussen 1931). In addition, my genealogical research indicates that there was considerable kinship connection between the two groups. Despite these similarities, the pattern of the seasonal economic cycle was quite different. Since the Back River people relied almost exclusively on caribou and fish, the largest aggregations occurred at the autumn caribou drives and at the summer fishing sites. A group chart that includes all the Utkuhiksalingmiut for 1924 resembles very closely the band charts that have been discussed above, though it is not certain that all these people ever formed a single aggregation.

The seasonal cycle of the Caribou Eskimo (Birket-Smith 1929, I: 134-7) resembled that of the Back River people in that they were most dispersed in winter and formed their largest aggregation (maximum size about 50 persons; Birket-Smith 1929, I: 74) in autumn in connection with caribou drives. More detailed information is available on their internal units than for the Utkuhiksalingmiut (Gabus 1944: 67-71; and interview material), which appear to closely resemble those of the Netsilik region. Probably this grouping often comprised the total local aggregation, especially in winter. It is difficult to assess the status of the band in this area. It may well be that the larger fall aggregations represented band assemblages similar to those of the winter sealing villages of the coastal Central Eskimo.

The Eskimo of the east coast of Hudson Bay and the south shore of Hudson Strait have been poorly described for the aboriginal era with respect to groupings or social organization in general (Balikci 1964: 88). However, the occurrence of breathing-hole sealing, summer sealing in kayaks, and autumn caribou hunting suggests a seasonal cycle and pattern of dispersal and aggregation that closely approximates that of the Iglulik Eskimo. Current accounts of family organization (Willmott 1961; Graburn 1964) in that area indicate that the nuclear family comprised the usual household unit, though there is no evidence that this was an aboriginal pattern.

The Eskimo of eastern and southern Baffin Island have been described by Boas (1888: 421-43) as adhering to a seasonal economic cycle resembling that of the Iglulik Eskimo. Sea mammal hunting, including here the pursuit of large whales from kayaks and umiaks, was a prominent summer activity. Large winter sealing aggregations appeared every year. Boas (1888: 425) estimated that the 1840 population of Cumberland Sound (before extensive depopulation) was split into eight villages of about 200 persons each which suggests larger band aggregations than farther west. Such a picture might be consistent with the good resource base in the eastern Baffin Island area. Two nuclear families generally inhabited each iglu (Boas 1888: 546).

The Polar Eskimo of northwest Greenland and the Eskimo of the coast of Labrador were transitional from the highly nomadic tent and snowhouse dwelling people of the Central Arctic to the more sedentary peoples of West Greenland.

Settlement along the Labrador coast was dispersed in small villages of permanent houses during the early part of winter. Later, breathing-hole sealing (which was not highly developed) and walrus hunting were carried on from snowhouse villages on the ice (J. G. Taylor 1965: 10-11). J. G. Taylor (1965: 44-5) designates three 'place groups' of 110, 270, and 360 which appear to correspond to the Central Eskimo bands in characteristics of name identification with a region, and periodic assemblage and fission. The largest aggregations did not, however, occur in the winter villages or later during the walrus and seal hunting season but rather in spring during cod fishing and in the summer when salmon were caught. An interesting feature of Labrador

Eskimo society was the very common occurrence of the polygynous joint family (J. G. Taylor 1965: 16ff).

The Polar Eskimo inhabited small villages of permanent shelters during the fall and the dark period of the winter. Ekblaw (1948: 3) found that these villages were populated by one to six nuclear families. In these small settlements, movement of people in visiting and longer term relocation appears to have been extensive. During the dark period, subsistence was based on stored food. The largest aggregations occurred in March and April when the entire population of 250-300 Polar Eskimo gathered in three villages. At that season, the time of greatest ceremonial activity, economic activity centred on walrus (Ekblaw 1948: 3). Since the three villages were close enough to communicate easily, it is difficult to decide whether they formed three separate bands or only one band. Summer found the Polar Eskimo split into small, relatively stationary hunting groups (Ekblaw 1948: 3).

Regarding the West Greenlanders, it appears from Birket-Smith's account (1924: 235-6) that the permanent winter villages were inhabited for periods of seven to nine months of the year. It seems that these villages were somewhat larger than the winter settlements of the Labrador and Polar Eskimo, and there was no larger aggregation of people during the year as they divided into small hunting groups during the summer period (Birket-Smith 1924: 130-2). There is little information available on the social organization of the West Greenlanders, but communal dwellings housed several nuclear families in contrast to the usual nuclear family household of the Polar Eskimo (Steensby 1910: 324-5).

Another type of settlement pattern was exhibited by the Angmagsalik Eskimo of East Greenland. The population of each winter settlement was housed within one large dwelling with an average of about thirty persons to a site (Holm 1914: 26). These 'long houses' were divided into living areas for each nuclear family. There is some evidence that members of each winter house-village were closely connected by kinship ties, possibly indicating a virilocal slant (Holm 1914: 36). The Angmagsalik lived mainly on stores during the winter with breathing-hole sealing practised but not highly developed as a group activity (Holm 1914: 50; Thalbitzer 1914: 399). They moved into tents in April but did not abandon the winter sites until later in the spring. *Uuttuq* sealing was the chief spring occupation. The largest aggregations took place in summer at points of land favourable to seal hunting from kayaks and whaling from umiaks when the entire population of the area (400 to 500 in the late nineteenth century) gathered in three villages (Thalbitzer 1914: 347). These aggregates appear to have resembled the band assemblage of the Central Eskimo winter sealing village and similar assemblages of the Polar and Labrador Eskimo.

Turning to the Western Eskimo area, the first group to be considered is the Mackenzie Eskimo, who appear to have been divided into two chief groups, each having rather large populations. The eastern group spent the autumn inland in small groups pursuing caribou. They generally returned to the permanent dwellings on the coast in early winter and lived mainly on cached food. In March they descended to the sea where they lived in snowhouses while hunting seals at breathing-holes. The breathing-hole sealing complex appears to have been highly developed. The eastern Mackenzie Eskimo are transitional in seasonal cycle and pattern of dispersal between the Copper Eskimo and the more westerly groups of Eskimo. The largest aggregation may have occurred during the breathing-hole sealing season in the manner of the Copper Eskimo (Stefansson 1914: 321, 348, 350, 378; Hohn 1963: 24-5).

The western group of Mackenzie Eskimo had as their centre the delta of the river from which they received their name. Although summer and autumn caribou hunts were important (Stefansson 1914: 163), the most significant resource was the beluga whale, hunted in late spring and summer to provide stores for the comparatively idle winter period. The largest aggregations were in summer in connection with whaling, (Stefansson 1914: 323). Permanent winter villages were quite variable in size (Stefansson 1914: 180). This group is the first to exhibit some of the social institutions prominent farther west. The *karigi* or *kazigi*, farther east, principally snowhouse dance halls, which appeared here as permanent structures, were not only meeting places for the men but also workshops, just as in North Alaska. The larger villages appear to have had two or three of these *kazigis* (Stefansson 1914: 170), perhaps associated with whaling boat crews and the *umialiq* tradition (Stefansson 1914: 168),

features that were also shared with North Alaska. Each village appears to have had a chief whose succession was patrilineal (Stefansson 1914: 172) reminiscent of the *isumataq* tradition of the East-Central Arctic. Permanent dwellings housed a number of nuclear families in most cases (Stefansson 1914: 166).

The Eskimo of the north coast of Alaska lived in permanent villages for about nine months of the year. The size of these aggregates appears to have been large for Eskimo groupings. For instance, two villages at Point Barrow had populations of 309 and 250 in 1852-53 (Spencer 1959: 367), and a census at Point Hope gives 276 for that community (VanStone 1962: 18). The year's economic cycle reached its climax in the spring whaling season. During the summer months, most of the people abandoned the winter villages, some to travel in small groups hunting the caribou, fishing, and fowling, and others to hunt walrus and bearded seal from boats (Spencer 1959: 367). Although some individual sealing either with harpoons or nets was practised, the fall and winter were periods of relative inactivity when stores from the spring and summer were used. The communities were segmented into nuclear family household units and umiak crews which were led in the hunt by a captain or *umialiq* and met at the *karigis* (Spencer 1959: 177-82).

The Nunamiut of the interior of North Alaska resemble the Caribou Eskimo in that the largest aggregations took place at the sites of the spring and autumn caribou drives (Spencer 1959: 132). Gubser (1965: 167) estimated that 50 to 150 persons normally comprised the band in that region, though he felt that occasionally as many as 300 or 400 might aggregate. Spencer (1959: 132) gives 200 to 300 as the size of maximal Nunamiut aggregations. After the caribou drives, ceremonial feasting and general festive activity prevailed for a period. The bands split into camps of one or two households inhabiting sod dwellings during the winter (Posposil and Laughlin 1963: 181). During the summer even greater fragmentation took place. Certain features of coastal culture, although phrased differently in the inland setting, were shared with the Nunamiut. Chief among these were the institutions of the *umialiq* and the *karigi*. The caribou drives were organized under the leadership of one *umialiq*, and there appears to have been only one *karigi* at each of the places of assemblage (Posposil 1964: 413). A virilocal slant has been noted for the Central Eskimo bands discussed in the main section of this paper. By contrast, Posposil (1964: 397, 412) indicates that matrilocality was the residential ideal and that this preference affected actual alignments.

Whaling and walrus hunting were the most important economic activities on St. Lawrence Island. The spring hunting of these animals provided the base for year round subsistence. Subsidiary economic pursuits included individualized breathing-hole sealing and walrus hunting, fowling, sea fishing, and collecting certain vegetable products (Hughes 1960: 101-31). Except during the summer fishing season, the population was concentrated in permanent villages. St. Lawrence Island social organization contrasted in several ways with that of the Alaskan mainland. One difference was the absence of the men's house, and another was the segmentation of the community into descent groups. Hughes (1958, 1960: 249ff) traces the appearance of patrilans to the centralization of the population. Groups converging in the modern settlements brought with them the name identification and social cohesion of the former sites of habitation. Patrilineages and patrilocally oriented 'kindreds' were also reported (Hughes 1960: 269ff). The boat crew was generally recruited within the clan and the 'kindred.'

St. Lawrence Islanders are most closely related culturally, linguistically, and, indeed, genealogically with the Siberian Eskimo. The latter were also mainly sea hunters with walrus and whale the most important products of the hunt (Hughes 1964: 3ff). Patrilineally organized boat crews provided the basis for the household unit (Hughes 1964: 8). Patrilineal clans and patrilocality (Hughes 1965: 42) were prominent features of the social organization, and their occurrence in Siberia as well as on St. Lawrence Island poses problems of diffusion, common heritage, or parallel development (Hughes 1965: 43).

On Nunivak Island the aboriginal population appears to have been variable in size (Lantis 1946: 162-3). The winter villages split into smaller segments for the spring seal hunt, which was the most important subsistence pursuit during the year. Other important hunts were fall sealing and summer caribou hunting. Whaling was not practised, and walrus were not so plentiful around Nunivak as in other Alaskan regions

(Lantis 1946: 173). The *karigi* was used as a men's house as well as a ceremonial centre but was not associated with a boat crew organization, as is the case in North Alaska. Residence was matri-patri-local as on St. Lawrence Island, and patrilineages had important social, religious, and legal functions (Lantis 1946: 239-44).

The Chugach Eskimo of the Pacific Coast of Alaska were blessed with a considerable variety of resources. Whaling and sea otter hunting were carried on at all seasons, though whaling reached its peak in winter. The most important seal and sea lion hunting took place in May. During the summer, fishing (especially for salmon) and gathering shellfish and edible plants were ascendant over hunting. Herring fishing and sea lion and mountain goat hunting were the chief autumn activities (Birket-Smith 1953: 24). Settlement, which appears to have been scattered for the aboriginal population of 500 to 600, was separated into eight place groups, some of which may have lived in more than one set of villages (Birket-Smith 1953: 20-2). Each village had a summer and a winter site, both featuring plank dwellings that housed several families. Whaling crews were made up of men from several villages (Birket-Smith 1953: 35). Other inter-village contact took place at the important annual Feast of the Dead and other ceremonial events (Birket-Smith 1953: 112). Each village had a chief, whose position was inherited patrilineally. Large numbers of slaves were kept (Birket-Smith 1953: 92-4).

Even though the data available on seasonal cycles, patterns of settlement, and social units in the Eskimo area are to a great extent inferential, there appears to be enough material available to make possible analysis of a number of problems.

These problems cannot be treated here. However, by way of summary it can be said that variations in adaptations, in types of settlement, and in features of social organization exist in an area inhabited by groups which share many elements of a common language and culture, where subsistence is devoted almost exclusively to hunting. The Eskimo area probably represents a fair segment of the entire spectrum of community types that will be found among hunters, and variations among Eskimo communities will accordingly repay our careful study.

DISCUSSION

SLOBODIN: Is it really true that kin ties and other social or ceremonial features depend on aggregations for their maintenance?

DAMAS: There has to be some kind of contact between people for this to occur.

SLOBODIN: Contact, yes, but necessarily contact en masse?

DAMAS: Perhaps when I used the word 'depends' in this case, it is a bit too strong. Probably I should have said that there is a better opportunity for diffusion throughout a region if there are periodic meetings of this kind. What I was really trying to point out was that in addition to the ecological reasons for the assembly there are social reasons as well.

HELM: You do feel that there is some sort of tribal identity, at least in terms of marriage, and yet the entire group which for practical purposes you call the 'tribe,' does not band together as a whole?

DAMAS: Yes. Returning to the idea of a band assemblage, I think, for instance for the Copper Eskimo, at least, there were at times a greater number of people gathered than could exploit the immediate area profitably, even with breathing-hole sealing. The motives of trade and ceremony were involved in these cases. These aggregations represented situations that would lie outside the range of adaptable harmony with the environment. People who joined in these larger camps would probably have to bring food into the camp with them.

McKENNAN: With regard to leadership in the bands, I am curious about the amount of direction required in the construction of *inuksuit*. Based on the long rows of stone structures that I saw in the Canadian Barren Grounds I would say that quite a lot of planning and leadership would have been required in their construction.

DAMAS: I doubt whether this would have been the case in the coastal Central Eskimo area. The *inuksuit* that I saw in the Copper Eskimo regions were very simple — mostly single stones set along ridges. In the Iglulik region I saw somewhat more elaborate structures. On the other hand, all the Central Eskimo *inuksuit* that I saw extended from a mile to two miles in length, not ten or twenty miles as I understand the case to be in Alaska.

HELM: It probably did not take leadership in the sense that anyone had to think it through. Decision was probably reached by a group of men in discussion.

WILLIAMS (to Damas): You have accounted for the influence of post-contact phenomena, such as disease brought by direct white contact, and its effect on local organization, but was there not whaling and sealing going on before the time of your censuses that probably decimated Eskimo populations?

DAMAS: I am not sure that this can be an automatic association. Returning to the figures that I gave earlier, the Iglulik Eskimo population was the one which had the greatest contact among the three groups considered, but

the numbers of Eskimo were practically identical in 1822 and 1922. There may have been fluctuation around the figure of approximately 500 which is given for those two dates, but this fluctuation was probably related to periods of famine. I did, however, mention one case where the sort of depopulation situation that Service mentions did occur. I did not find such occurrences to be general in the remainder of the area.

MCKENNAN: Did Service not argue that depopulation is the result of acculturation, whereas there has from earliest times probably been similar depopulation resulting from famine and warfare.

WILLIAMS: I am wondering if this depopulation, which is the result of acculturation, is quite the same thing as the occasional famine situation, because if the normal population grows at three or four per cent each year, famines cannot occur too often or there would simply be continuing depopulation. If famines are not too frequent, decline can perhaps be controlled. At the same time, expansion above an optimum population level might be checked by the practice of infanticide.

DAMAS: I would agree in general with what you have said, but infanticide must be thought of as an individual matter — that is, a measure designed to control the number of offspring within a single family — a measure that is not designed to improve the population-resource balance in the band as a whole.

MCKENNAN: I think there is something comparable to a one-crop economy inherent in some hunting situations. For instance, in the case of the Back River people in 1958, when their one crop, in this case caribou, failed, the people starved.

HELM: We northern ethnologists encounter situations where people actually starve, in contrast to ethnologists working with other band societies as, for instance, those working with the Tiwi, where the Tiwi population may be low but is at a uniform population level. They are not in an ecological situation where half their number might be depleted in a single year. Only a single month without food is necessary to eliminate half of the population in a given area. Quotations from the early contact period in the Mackenzie area might be helpful in this context. In 1811, which was twenty years after Mackenzie's visit to the area, Wentzel from Fort Simpson reports as follows:

I am unable to describe my own position. All my Indians have starved more or less . . . April 30 — From one small band only I received news yesterday that five are dead from hunger but from the majority of natives I have not heard since November. They were already at that period gnawing at their clothing they had upon themselves. Hares have failed totally [this is in relation to the 10-year rabbit cycle] and the large cattle have been uncommonly scarce at this place in particular. The cold this winter has been the severest that I've ever known. The ice at the Grand River [Mackenzie] is no less than 4½ feet thick. At this late date none of the snow has disappeared from the woods.

In other words, the ice was too thick for the natives to penetrate it with horn chisels, the hare failed, and the caribou did not come through. In the 1830's

King says that "forty to fifty died of starvation" at Fort Resolution. There probably were not more than 200 Indians around that settlement.

DAMAS: Of course all of this occurred in a post-contact period. Had there not been a depletion of resources by that time, or do you think that this sort of famine situation had been occurring prior to contact?

HELM: I have tried to reckon with ways that white men could have caused this situation, but the Indians appear to have been better off with regard to equipment than they were aboriginally.

DAMAS: With respect to the Eskimo, there have been cases where depletion of game resulted after the introduction of the rifle.

HELM: There was perhaps one rifle in each band in 1811. Actually there is no reference to starvation in the Mackenzie area after about 1870, and by that time there were enough guns available so that real slaughtering of animals could have taken place.

ROGERS: In the James Bay area in the 1880's there were times that large numbers of Indians in particular river valleys were wiped out by starvation. At Fort Chimo in 1889 about one-third of the Naskapi starved to death.

DAMAS: I think that the main problem here is that Service tries to show that the sort of depopulation situation that was due to epidemics, which, I found, probably occurred at Bathurst Inlet, was common throughout much of the hunting and gathering world at contact. We have indicated that for northern North America, at least, cycles of population fluctuation probably always occurred. The period of population slump could have created the compositeness in band structure that he describes. At the same time Helm's evidence and my own seem to indicate that reconstitution took place so that in time the bands were again kin-linked entities. As long as band exogamy is not the rule, and it is not in these two areas, marriage will occur within the band and quickly override the compositeness.

THE BIRHOR OF HAZARIBAGH

By B. J. WILLIAMS

RÉSUMÉ

Les Birhors sont un groupe de chasseurs-cueilleurs du centre-est de l'Inde, qui vivent aujourd'hui à proximité des villages de cultivateurs avec qui ils font un commerce actif. L'unité corésidante, la bande, est organisée autour de la descendance suivant la lignée paternelle, qui présente toutes les caractéristiques d'un lignage. Selon des données ethnographiques qui montrent: a) que les dimensions de la bande sont extrêmement flexibles et qu'il n'y a, par conséquent, aucune discontinuité sociale due aux variations saisonnières des ressources; b) que la fréquentation habituelle et prolongée d'un territoire de chasse donné ne semble pas être le facteur le plus important en ce qui concerne le succès personnel du chasseur, et ne peut donc être retenue comme une raison qui justifie la résidence dans le lieu de la lignée paternelle; c) que le nombre maximum de personnes faisant annuellement partie du groupe est sans rapport avec les impératifs technologiques de subsistance.

As the time for this presentation is limited, I shall only discuss two items of importance to local organization among the Birhor, a group of people still engaged in hunting and gathering activities in East-Central India. These are: first, an analysis of band composition; and second, an analysis of band movement. With regard to band composition I would like to present evidence on two points: (1) The band, which has as its core a patrilineal descent unit, is extremely flexible in size and therefore is not disrupted by seasonal variability in resources; (2) The descent unit characterizing the band is best termed a lineage.

With regard to band composition two points which I hope to make are as follows: (1) extended familiarity with a local hunting area does not appear to influence hunting efficiency and therefore cannot constitute a reason for patrilocal residence, and (2) maximum annual group size is not a function of the requirements of the subsistence technology.

Since there is no monograph on this group, the Birhor of Hazaribagh District, Bihar, I shall devote a few minutes to a general description of the Birhor and their environs. The majority of Birhor live in the Chota Nagpur region of South Bihar, India. S. C. Roy published a monograph on the Birhor of the Ranchi District in the 1920's. Since then some studies of the Birhor have been carried out by scholars from Ranchi University and by the Anthropological Survey of India. B. K. Sen published two articles on the Birhor of Hazaribagh prior to my own investigations there in 1961-62.

The Hazaribagh Birhor subsist almost totally from hunting and gathering activities. They are not a 'pristine' hunting group, however, as they live in an area densely inhabited by agriculturalists with whom they daily exchange products. Although the Birhor do not live in and are not attached to agricultural villages, they are never far from such villages.

Agricultural land is being cleared at the expense of forest at a fairly rapid rate throughout the district, but most villages are still bordered by uncleared forest and there are large tracts of rough terrain unsuited to agriculture. It is in these areas that the Birhor continue to hunt and to gather forest products. Economically, the most important game animal is the hare, which is caught by being driven into nets. Almost all hare are traded to the villagers for rice. Other forms of game are eaten by the Birhor. The Birhor also make ropes from inner-bark fibre of certain trees and vines, which they also trade for rice. Rope making is of considerable economic importance for these people only during the wet season, at which time it is equal in importance to hunting.

Of approximately thirty bands visited, only four had planted crops during the past year, and these were bands that had been 'settled' in a government welfare program within the previous two years. Most Birhor bands attempt to keep chickens and, occasionally, goats, but these are eaten only on ceremonial occasions and are not part of the every-day economy.

The statement seems then to be justified that, directly or indirectly, Birhor subsistence is dependent almost totally on hunting and gathering activities. Since they have a parabolic relationship with village agriculturalists, however, there are undoubtedly forces influencing them which would not be present in an area uniformly inhabited by hunting-gathering groups. And certainly there are forces not present which might exist in the hypothetical situation. For example, the Birhor as a small, enclaved minority are not politically autonomous and are not in a position to territorially exclude anyone from any place even if they chose to do so.

The Birhor are migratory, and, although not territorial in the usual sense of the term, they do tend to remain within a prescribed area. This area has no clear boundaries, but it does have a focus. That is, if Birhor are questioned about residence in a general way, they will identify it with the place where they usually spend the dry-season months, March through June.

The Birhor band fluctuates in size, and therefore the number of bands in an area fluctuates with seasonal changes. Bands come together in largest aggregates during the dry season. They may remain together during the subsequent wet season, but if they have to move from their dry-season camp they often split for the wet season. At the end of the wet season, bands split into the smallest average size of the year. The co-resident unit at this time is occasionally found to be the single household for only a short period. From this time and throughout the cold season, October to February, the bands begin to aggregate into slightly larger average units. They begin to move back to the dry-season camp around March. These, of course, are overall patterns, which are not demonstrated by all groups in a given year.

If it is not obvious at this point, then I should explicitly state that throughout this paper I have attempted to use the term 'band' to designate a residential unit, and that only. The band is the residential unit in which day-to-day cooperative activities, usually hunting, occur. This is not to take exception to the use of 'band' or 'band-level' as a classification of overall societal type. It has proved useful, however, in analysing my data on the Birhor to be able to discuss residence units as separable entities from descent units. There is ample precedence for using band in this restricted

sense. I might note that the Birhor have neither a term designating what I have here called the band, nor a term to designate the camp-site other than to refer to it as 'the clearing' or 'the edge of the clearing.'

The band is always composed of relatives. If the ideal of sons remaining with their father after marriage is followed, then bands will be constituted of patrilineally related males and their families. In the field census taken in early 1961 within 25 bands, it was found that 13, just over half of the total, had only one lineage represented; eight had two lineages; three had three lineages; and one band was found with four lineages represented. The latter was atypical in many ways, being unusually large and semi-settled. Since this is census data, it represents the *de facto* living arrangements including families which are involved in extended visits or who otherwise do not feel themselves permanently attached.

It should be noted that this distribution of lineages in bands does not represent a frequent or flagrant departure from the Birhor ideal. The ideal is that when a man marries he remains in the same band with his father. This ideal is invariably met with respect to ego's 'own' father. It is not always met with respect to classificatory fathers. Therefore, if ego's 'own' father dies prior to ego's marriage, he is much more likely (than otherwise) to be found living matrilocally, even if he considers it a temporary arrangement. In almost all cases, matrilocal residence is initially considered to be temporary, although it obviously becomes permanent for some.

Speaking of matrilocal versus patrilocal residence in this context simply means that we found only instances of the Birhor residing with the male's natal band or with the female's natal band. Theoretically they may reside in any band in which they have relatives. This could occur from the visiting patterns which allow a family to visit any band in which they have relatives. When a family visits, as contrasted to an individual on a visiting trip, it hunts with the host band and maintains its own cooking hearth, as does any other household. Such a visit always holds the possibility of grading insensibly into residence. Therefore it follows that visiting patterns establish the limits of possible residence.

Since residence in the female's former band is the only alternative to patrilocal residence actually followed, it is important to determine what factors cause a visit to the wife's former band to become fixed as residence. Two major categories of factors seem to be involved. The first is variation in resources, both hunted and collected items. The second is variation in the distribution of births and deaths. In some ways the latter set of circumstances is by far the most important.

With regard to variation in resources, temporary fluctuations in game animal supply seem to affect visiting patterns slightly. Some visiting families occasionally prolong visits and acknowledge this to be the result of good hunting in the area.

That this factor does not have greater influence than it does seems related to certain features of the social system. Sex and age are the two most important dimensions of authority in Birhor society. The male household head expects to exert a great deal of authority in his own family. Such a man will find this authority weakened and frequently challenged if he lives matrilocally. Disputes, even relatively serious ones, are seldom verbal and

direct, especially among the males. However, small disputes and interpersonal manoeuvrings are continuous, and even if submerged much of the time they will frequently irritate the man living matrilocally. Brothers will usually close ranks in such issues vis-à-vis a male in-law. Therefore a man who continues to live matrilocally must be content with what must be considered a lower and less enviable status within the band than if he were living with his own brothers. Consequently, differences in hunting yield seldom (to my knowledge, never) result alone in permanent moves.

This is not the case with the movement of entire bands. The bands do try to stay in a familiar area, although they do drift slowly. One band was known to have moved more than 75 miles in three generations. This much time depth is not available in the data for most bands, but this case seems to exhibit greater movement than usual, and small cultural differences today differentiate it from other bands in the surrounding area. Clines of cultural differences in ceremonial practice, beliefs, and dialect, generally in a northeast-southwest direction, indicate a fair degree of stability for most groups. The drift of bands that does occur very probably has economic causes, primarily the increase of cultivation and the consequent decrease in hunting area.

Chance factors in mortality and fertility are perhaps the most important forces contributing to the permanent displacement of males from their natal bands to those of their wives. For example, arranging a marriage for a young man is extremely difficult and, by Birhor standards, expensive. If a young man's own father is dead, his father's brothers are expected to arrange his marriage. However, the ideal and the practice are never identical in this case, and if sufficient help is not forthcoming, a young man has two possible alternatives: he may marry an elderly widow, or he may accept matrilocal residence in return for a wife. As a single cause of permanent matrilocal residence, this is probably the most common.

A band in which a number of deaths occur in a short space of time is likely to split. Since supernatural forces are believed to be involved in all deaths, with the possible exception of the newborn, suspicions of witchcraft are always engendered by important deaths. Although I have only limited evidence to support this opinion, I suspect such suspicions fall along pre-existent factional lines. This can cause and has caused bands to split, and one or more families to take up residence with the female's relatives.

As another instance, death or low fertility can simply reduce the size of a patrilineally related group to the point that they do not constitute a socially viable band. That is, they cannot handle the arrangements necessary for contracting marriages or handle emergencies such as the prolonged illness of more than one adult. By being unable to handle these, I refer to the fact that aid is normally extended only among those persons who band together at least part of the year. Therefore, if a group is not of sufficient size to handle its problems, then families will tend to reside more and more of the year with a band who can assist them. Matrilaterally affiliated bands are those usually available in such circumstances. The Birhor explicitly recognize this problem. The smallest band with which I worked intensively consisted, usually, of four households, one of which was that of a matrilocally resident son-in-law. The other members of the band were actively

striving with every means at their disposal to make the son-in-law feel obligated to remain with them. They explicitly recognized that the group would be too small to do well without him.

Band composition of the Birhor is determined in part by the desideratum of maintaining a minimum-sized social unit. The average band size ($n=25$) for the Birhor is only slightly above this minimum and was found to be 26 individuals. The mean for the three- and four-lineage bands is higher than the average, 38.5 individuals per band. Twenty-one of the 25 bands were either one- or two-lineage bands. Of 12 one-lineage bands (one case being deleted because of inconsistent and suspect data on the number of lineages) the mean size was 24.1 individuals. Of eight two-lineage bands the mean size was 23 individuals. This further indicates that multiple lineage bands are not a function of local differences in resources but an attempt to maintain a minimum acceptable band size in the face of random fluctuations in the size of any given descent line. It must be assumed that the minimum for the average 'stable' unit is somewhere in the range of 20 to 25 individuals.

I have been calling the descent unit which forms the core, and most often the entirety of the band, a 'lineage.' It obviously is not a clan, although the Birhor do use names which are identical to Santhal and Munda clan names to designate the patriline. These names are not used to establish kinship. In no case were they known to have been evoked to establish social solidarity. Moreover, members of the same patriline in different bands were observed to be using different ones from among these names when asked. Also, the same name was found to be used by lineages claiming to be unrelated, and in one case a marriage took place between people of such lineages. The major function of such a name seems to be prestige with outsiders. Persons should have clan names.

On the other hand, these bands, at least at their average maximum annual size, cannot be considered simply lineal or extended families (using lineal and extended as synonyms). The families within this maximal band do not form a single household in the sense of participation in a household economy. In addition, the average band is almost twice the size that can be explained as a sequence of co-resident families of procreation representing, at most, three generations of individuals.

Since the descent unit larger than the family does not meet the criteria to be called a clan, it is best termed a 'lineage.' Within the lineage, kinship is reckoned not by the recitation of genealogies, which is prevented by a great many name tabus, but through the existence of common relatives. This means that lineages are restricted in range, tending to be localized in small areas.

In some instances the lineage constitutes more than one band even during the dry season. In such cases visits by individual males or families occur during the year. It is my guess, but only a guess, that these represent lineages in the process of splitting. Some may coalesce later. Others, if visiting becomes infrequent, will eventually cease to be kin.

The lineage has little symbolic objectification except in what might be called covert forms. These reside in the inheritance, through the males, of a common pantheon, common ritual with regard to this pantheon, and

common sacred mountains. There are differences between lineages in one or all of these items.

The lineage is, of course, the unit within which economic aid is sought. This aid becomes necessary in the case of prolonged illness and at some other times, but the most common situation is that in which men are trying to arrange for brides for their sons. The search for a bride and the wedding ceremony itself always require the help of lineage members. Normally this coincides with the group which bands together for at least part of the year. In the only case observed, a man in seeking aid from lineage members in a separate band eventually had to change his residence to their group to secure any promise of help.

Data on band and family movement, gained retrospectively through informants, were almost always inaccurate with regard to moves which had taken place a year or more earlier. Many errors appear in recalling the numbers of moves that had been made. Therefore the figures mentioned in the following statements refer only to the movements of two bands during the period in which they were under observation.

All moves of these two bands were recorded for a period of fourteen months. There were 15 households which were present in the two bands through most of this period. Other households of relatives joined and left these households during the stated period. Confining ourselves only to major moves (where women and children cannot walk easily back and forth during the day and where the entire band moves, usually) and counting major moves per household, we find that the 15 households moved 136 times. This would be about 9 moves per household during this time or about 7.8 moves per year per household. The mean map distance involved was 6.2 miles per move per household.

In this sample of 136 household moves made during the period, there were 12 which because of their timing were clearly motivated by non-economic factors and 124 which also because of timing were classifiable as regular seasonal moves or within-seasonal moves to improve hunting. In other words, 91 per cent of the moves were primarily the result of economic factors. Furthermore, among the Birhor, it is the distribution of hunted game which is specifically considered in deciding when and where to move. The distribution of collected items only appeared to be a factor in two cases.

It is probably true that not only the frequency of moves but also the average distance moved is a function of hunting. Only in very rare instances would a hunting party walk for more than an hour, about 3 to 4 miles, before beginning to hunt. The average distance between moves is 6.2 map-distance miles, roughly twice the radius that a hunting party proceeds from any one camp. Of course, this is in terms of mean distances, and there is considerable variation in distance between specific moves.

It has been suggested that patrilocality in hunters may be a result of the necessity of a hunter knowing his territory. This does not seem to be true in the case of the Birhor. Although they are patrilocal and do try to remain in a familiar area, it is only important that someone in the hunting party knows the area. Given that at least one man in the party knows the area, differences in hunting efficiency appear to be a function of a man's knowledge and skill with respect to the game, regardless of the area.

Since the proper conditions for testing this hypothesis are not often met on Birhor hunts, as few men will be found hunting in unfamiliar territory, the sample size is small. Data were recorded on 14 hunts in which (1) success was recorded as bag per individual, and (2) there were persons in the hunting party familiar with the area as well as those unfamiliar with the area.

<i>Unfamiliar</i>	<i>Familiar</i>
mean success $\bar{x} = .22$	mean success $\bar{x} = .30$
N = 60 man days	N = 27 man days
$s^2 = .24$	$s^2 = .28$
Standard error of the difference of means, $s = .119$	
$t = .664$, non-significant at the 5% level.	

However, counting the successes of one young man who was a good hunter, it was found that he got five animals in six hunts in unfamiliar territory ($\bar{x} = .83$) and four animals in six hunts in familiar territory ($\bar{x} = .67$) — both of which are significantly above the mean of the others, whether in familiar or unfamiliar territory.

Therefore, the statement "differences in hunting efficiency appear to be a function of a man's knowledge and skill with respect to the game and regardless of the area" seems justified.

The last point concerns seasonal movements and maximum band size. Bands aggregate to their maximum average size during the dry season, from the end of February to mid-June. This is not because of any requirement of their hunting technology. The Bihors prefer to hunt in groups which do not exceed seven or eight nets. With this number it will often happen that one or two nets cannot be put up on particular drives because there is not enough clear space for them. Five nets appear to be just as efficient as seven or eight and can be fielded by only three households. When more than one band camps together, they still hunt in small parties. A large party tends to split up in the forest.

These larger bands stay together during the wet season when the hunting is not good because they can substitute rope making and the more abundant collected foods for the decreased yield of the hunt. They tend to split into smaller bands at the end of the wet season, when forced to by economic circumstances. In other words, they aggregate when they can, they split when it becomes necessary.

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APPENDIX

The following remarks are added to provide additional material on the Birhor so as to increase the comparability of this presentation with others given during the preceding part of the Conference.

Two kinds of leadership, or at least two positions, can be distinguished among the Birhor. One position, not named or formal, is that of hunt leader. Its existence is apparent only from observing the hunt. The hunt leader is usually the eldest male who hunts daily. He leads by suggestion rather than by order. When a decision is to be made on a hunt, anyone and everyone may casually volunteer an opinion, but it is the opinion of the eldest male, just as casually advanced, that is usually followed.

The other type of leadership, bearing the name *pahan*, is a ritual or ceremonial position. The *pahan* is a diviner and curer. His services are utilized in the case of sickness or, often, snakebite. He is the person who is consulted if the group does not get any game for a long time. In this case he will decide which god or gods have been offended and will prescribe the proper sacrifices to correct matters.

It is a little difficult in some instances to decide who is a *pahan* because no one will claim to be the *pahan*. Yet if you ask a boy, "Is your father a *pahan*?" he will reply, "he is *pahan*-like." In effect every household head is *pahan*-like. Every household head can perform the ceremonies, and of course this is necessary when they split into smaller groups. However, there is one man in each lineage more highly respected for his abilities than other household heads. Other lineage members will come to him when plagued by serious difficulties, even if they are living in another band. This man, then, is the *pahan*. A very well-known *pahan* will be patronized by persons of other lineages and even by villagers.

So far as I know, the *pahan* is never the hunt leader because he stays in camp too much and does not know intimately what has happened on the previous hunt. This disqualifies him as a hunt leader, since the almost exclusive topic of discussion on any hunt is the previous day's hunt, the behaviour of the game, and all previous hunts in the area.

There was one *pahan* in particular (in this case called *bhagat* — a term roughly equivalent to *pahan*) who enjoyed the exercise of authority more than was typical for the Birhor. On a hunt he would make a suggestion and go off in one direction, and the regular hunt leader, with all others following, would go off in another direction. The obvious status conflict was a source of amusement to the young men and undoubtedly served as a force to keep this *pahan* engaged in ritual duties in camp as much of the time as he could afford. This, therefore, increased the differentiation of the two positions.

So far as fission and fusion are concerned, the Birhor visit only with relatives. They say that they can visit with friends or with relatives, but I could not find any cases in which these were not the same. In the fusion process if a band has split earlier in the year with the expectation of reuniting at a later season, they will, when they reunite, with little ado move in directly, building their huts in close proximity. If they are a visiting group and do not regularly band together during the year, they will never move in so directly but will build a camp 100 to 500 yards away. The men will hunt together for a few days or a couple of weeks and then perhaps move in to a position of less differentiated proximity. I think this is a feeling-out period. If smooth relationships do not develop properly, the groups simply do not move in together.

In the fission process one can almost tell by walking through a camp which groups are going to split in a subsequent move. There are hints of this in the placement and sometimes in the orientation of the huts. I mapped all camp-sites after their abandonment. Clusterings in the house patterns indicated fairly well which families would be together at the next site. Though representing long-standing sociometric relationships within the band, these clusterings are also influenced by disputes which take place during the year. Most disputes are resolved by splitting when it is time for the band to move and by coalescing at a later season when the dispute is no longer important.

There is a struggle to keep offspring, both sons and daughters, within the band. Each man tries to keep his son with him after the son's marriage. He also attempts

to keep his daughter but is more willing to give up his daughter than his son. This is one reason that marriages are difficult to arrange. A great deal of negotiation is involved, and many negotiations fail. For this reason, although they have about eight named forms of marriage, one form of marriage is found almost always to occur among first marriages. This is a direct exchange marriage between lineages. In each case a daughter is exchanged for a daughter-in-law. To do this, they often utilize lineage members outside the nuclear family to procure a bride for the son. In each marriage the same kind of struggle occurs and is almost always resolved in the same manner resulting in patrilocal residence.

EGGAN: This is perhaps one of the strongest considerations in the arrangement of marriage.

WILLIAMS: Yes, this is the reason why there has to be a fair-sized band. It is necessary to have lineage members to help you, since most nuclear families do not have a daughter at the right age to exchange for a daughter-in-law. Besides, relatives are necessary in the marriage negotiations and in helping with marriage expenses.

In connection with marriage, it should be observed that there is a rule that says simply you do not marry a relative. The feeling is extended to a non-relative who for some reason is raised in the band. He is felt to be a kind of kin, and kin terms are extended to him. A person just does not marry within the band.

I would like to add a point, not in reply to Dr. Eggan's statement but which I think should be included here. After considering some of the other papers I feel that there are great differences between the Birhor and, for example, the Naskapi. It is very difficult for a Birhor to change bands and it is inconceivable that he would do so because he was bored. With the exception of the wife's band, the other bands are in a sense alien. As a matter of fact, this is one of the difficulties of field-work — you can spend months working your way into one band, but this provides you with no entry at all into another band. It does not mean a thing that you can talk about Joe so-and-so, even though he is a relative. It is necessary to spend an equal amount of time establishing rapport in the new band. Bands are fairly autonomous in this respect.

DISCUSSION

BICCHIERI: You have said that in cases where groups fall below a certain population level because of death or other reasons, the band is reconstituted in order to make it a viable economic unit. I understand your social arguments, but I cannot see how you can separate these from the economic ones.

WILLIAMS: There are two economic factors that are involved: (1) the existence of resources in one area, and (2) the ability to exploit them. The existence of resources on a slightly higher level does not result in the appearance of multi-lineage bands.

EGGAN: With regard to these multi-lineage bands, are they made up of two parallel lineages or merely a lineage with one or two more cases of uxorilocal residence?

WILLIAMS: In cases where there is one family which is not a member of the lineage, I have classed the band as multi-lineage. In fact most of them are cases with simply one family living matrilocally. There were probably 20 per cent of the families living matrilocally in the entire area.

EGGAN: I wonder whether it would not be better to use a term other than 'multi-lineage' if that is the case. Perhaps 'pure and impure' lineages.

HELM: For what period of time did the bands endure, fifteen to twenty-five years?

WILLIAMS: Yes, for at least that long. Of course that would be the core of the band, for there are always a few families that shift to other bands.

HELM: Once they have changed, are they assumed to be permanent members of the other band?

WILLIAMS: Yes.

DAMAS: I am trying to identify the units that you refer to as bands. To me they seem to be kinds of extended families — minimal hunting groups. Would you say that generally four or five of these groups and generally the same ones aggregate at some point in the year's cycle? If so, I would tend to call the larger aggregation the 'band.'

WILLIAMS: Five of the twenty-five bands in the area usually aggregated during the dry season. This would comprise a gathering of a couple of hundred people. This particular group lived in an exceedingly rich area, the reason for the richness being that the area occupied was just south of a region which had formerly been a private hunting preserve of a rajah. I believe that this sort of aggregation of what I have been calling 'bands' into larger groups might occur more often if it were allowed by resource conditions. Generally contact between the bands comes about through intermarriage and visiting.

Intermarriage works as follows: once a group is found in which there are a number of unrelated females, marriages between the two bands will occur. The entire generation is likely to exchange because the people are

then known. The nearest band with a number of members unrelated to members of your band is the one with which marriages will be contracted. You are likely to be linked to all the other surrounding tribes through kinship because of this practice.

EGGAN: In the next generation they would really have to marry into some other band.

WILLIAMS: Yes.

DAMAS: Returning to the problem of distribution of bands, am I to understand that two bands which were without a very close tie between them might at times be residing in the same clearing in several neighbourhoods or compounds? What happens in cases where bands are more closely related or have more highly developed visiting patterns? Do they camp together in one village or maintain separate compounds or neighbourhoods?

WILLIAMS: In the case of the five bands that come together each year in a common clearing, the camp is joined, but in another case that I can mention the bands were separated into clusters about 150 yards apart. In that instance the two bands shared a clearing for an entire dry season. A woman who was the dominant character in one of the bands was the sister of the leader of the other.

DAMAS: The situation where the five bands aggregate, comprising a group of approximately 200 persons, seems to correspond to what I have been calling a 'band' in the Central Eskimo area, though your group is somewhat larger than my average of 100. I would call the gathering of these five bands 'the band aggregation,' and the constituent units that you are calling bands, 'hunting groups.' But your situation might not be as structured as mine.

WILLIAMS: I think that it would be if economic conditions were favourable. There is another point in this connection which I have not previously mentioned. That is, there are sacred mountains which bands relate to. Two of these are very impressive, large forested tracts. There are annual hunting parties on these mountains for some groups, and related bands hunt together in these localities. There is a correspondence under these circumstances with your sealing villages.

DAMAS: Could you construct the sort of genealogical diagram that I had for the sealing aggregation, which would form a fairly continuous kinship linkage?

WILLIAMS: You could find kinship terms that would be used among all the members of such an aggregation. The genealogical picture might be somewhat complicated, however, because of conflicting genealogies given by different generations, but you could link up everybody; they are kin.

DAMAS: Is there ceremonial activity accompanying these periodic fusions? Is there a significant affirmation of ties through ceremony? You recall that I had spouse exchange and association through dancing.

WILLIAMS: The dances would occur when the band is at the largest size, but there are no other ceremonial partnerships.

PALIYAN SOCIAL STRUCTURE

By PETER M. GARDNER

RÉSUMÉ

Les Paliyans de l'Inde du Sud sont des cueilleurs de nourriture caractérisés, par rapport à leur composition sociale, par trois traits dont dépendent tous les autres: la structure bilatérale, la réciprocité des relations entre personnes et la proscription de l'agression délibérée. Les deux derniers traits, tout particulièrement, expliquent les bases sur lesquelles s'établit la classification de la parenté, et les moyens que les Paliyans utilisent pour contrôler le fonctionnement de leur société; ils rendent aussi compte du fait que les Paliyans se sont constitués en groupes fluides et peu nombreux. La violation des normes de l'un ou l'autre de ces deux traits est appelée «un manque d'égards». Leur milieu naturel et l'environnement des différentes cultures avec lesquelles ils sont en relation exercent sur les Paliyans une pression considérable à cause des variables qui commandent le cours de leur établissement et à cause des réactions psychologiques qu'entraîne un harcèlement chronique. La pression exercée par leur milieu naturel a influencé le développement de la structure bilatérale; la pression qui résulte des contacts avec les cultures voisines a abouti à la réciprocité et à la pratique de la non-agression. Une étude consacrée à vingt-sept cultures de peuples cueilleurs de nourriture montre la récurrence des mêmes traits dans des milieux similaires (des cas intermédiaires corroborant les prévisions), ce qui tendrait à étayer l'explication écologique qui en est proposée. Les Paliyans et les populations qui leur ressemblent constituent un type bien défini de cueilleurs de nourriture réfugiés. On ne possède guère de données qui permettent de croire à l'existence d'une lignée asiatique de cueilleurs de nourriture. La typologie des cueilleurs proposée par Service ne paraît pas adéquate.

INTRODUCTION

The Paliyans, a food-gathering tribe of Madras, South India, represent a particular social-structural type which recurs among other tribal peoples of India and among many food gatherers elsewhere. This structure is based upon bilateralism, symmetry in interpersonal relations, and the avoidance of *overt* aggression (which will be abbreviated, although not strictly correctly, as aggression-avoidance). Because the composition and organization of Paliyan bands are determined largely by these three features of social structure, this paper will concentrate first on their description.

Since widespread recurrence of the same structural features prompts the question of causality, the second part of this paper contains an examination of whether these three features represent convergent adjustments to natural environment, similar responses to culture contact, or, in the South Asian cases, a common cultural heritage. None of these possibilities can be easily dismissed.

The Paliyans live on the lower eastern slope of the hills which divide Madras from Kerala, a 5,000- to 8,000-foot high spine down the southernmost 150 miles of peninsular India. While they speak a dialect of plains Tamil

(the language of the majority of the 30 million people of Madras State), Paliyans are physically distinct from their Tamil neighbours. Instead, their various physical types fall within the range of South and Southeast Asian Australoid tribal types, formerly termed Negrito, Malid, Veddid, and proto-Australoid. They are most like the Semang of Malaya and other Indian gatherers (*see especially* Evans 1937; Fürer-Haimendorf 1945: 35-7; Schebesta 1927; Skeat and Blagden 1906; *see also* Coon 1958: 29; Fürer-Haimendorf 1943: 17; Olivier 1961: 274-5; Sharma 1963). Culturally too, they resemble the Semang, the Kadar, the Malapandaram, and the Chenchu.

Today, only a few of the approximately 3,000 Paliyans are not involved in contract labour or agriculture. These few continue their traditional subsistence pursuits, moving in bands not larger than 20 to 30 persons, gathering *Dioscorea* yams, sago, honey, and small, slow game. There is good reason to believe, however, that economic interaction with plains Tamils is a pattern that goes back millenia. Therefore, it is most misleading to speak of naïve, aboriginal, or pristinely primitive cultural features for the Paliyan.

PALIYAN SOCIAL STRUCTURE

The three structural features—bilateralism, symmetry, and aggression-avoidance—can best be understood when social structure is considered from the various standpoints of (a) social aggregates, (b) kin classification, (c) means of social control, and (d) catharsis.

(a) Social aggregates

The social aggregates of importance in Paliyan society are the nuclear family, bilaterally extended groupings of kinsmen of various sizes, villages, and chains of villages.

The nuclear family is the only unit of Paliyan society characterized by cooperation. Even so, within the family, division of labour is not marked, and marriage is regarded as a necessity only for those who are unable to provide for themselves. A conspicuous number of people never marry, and many, even the elderly, live alone for extended periods. Normally, however, the marital pair constitutes the core of a residentially, sexually, procreatively, and economically distinct family group. And the marital pair is the usual locus of sharing in these various areas. On the other hand, marriage (*kalyānam*) may involve no sharing, as shown by one instance in which a couple, though living together, had exchanged no food in four months. Each spouse had provided his or her own food, though both had helped feed the children. Sexual liaisons, which are common, can only be distinguished from the fragile, short term, serial marriages in that they are secret rather than public.

Ten per cent of marriages evolve without ceremony out of adopted parent-child relationships (variations of this pattern have been reported: Whiting 1950; Burling 1963). Less than five per cent of marriages are polyandrous or polygynous.

Although village membership is always in flux, a couple appear to choose to reside only in villages in which one spouse already has a primary relative (*see* Appendix II). A young couple claim to the outsider that they prefer living in the village of the man's natal family, but they live almost as fre-

quently with the woman's relatives and are free to reside anywhere. The unity of the resulting aggregations of kin is mainly spatial (transiently) and effective (between given pairs of families but not among members as a whole). The basis for calling them kin groups is therefore inadequate. The component nuclear families only work parallelly, and except for emergencies, they are economically independent. One characteristic form of interaction among such bilateral clusters of relatives is the practice of walking to and from work together. Interaction was observed to diminish rapidly among tertiary and more distant relatives. Further, certain closer classes (such as a man's brother) are avoided. The largest group exhibiting much interaction is roughly co-extensive with the group called *sondakārar*, or relatives. This is the usual residential unit or band of nomadic Paliyans, which numbers from 18 to 30 persons. This unit retains its special meaning today when bands frequently coalesce into larger villages. Village-wide interaction takes place only in certain contexts: during snake alarms, games, hunts, and ceremonies. Even then, there is little cooperation and no leadership. The occasions expected to unify do not necessarily function in that way for a whole community; for example, the annual ceremony dedicated to village deities drew only a third of a village (although, in the case cited, every absentee had a primary relative present). Wedding feasts and funerals alone attract everyone.

Most villages have one or two headmen who use wit or diplomacy to avert conflicts. Having no mandate to punish, order, or arbitrate, they constitute a social lubricant. The conciliator's role is one which is created slowly over time, as an individual is found to be effective in creating the good will that resolves disputes.

Although Paliyans consider the joint aggregate of their own village plus the immediately neighbouring villages a discrete group, this is purely relative (in the manner of kindreds). Members of each village marry and interact with the members of immediately adjacent villages along the linear ranges creating chains of overlapping so-called territories rather than actual distinct territories. There are no fixed village boundaries, and economic need is justification for migrations. Migrants sometimes even become headmen. Aggregates at all levels of organization, including the family, are affected by the ideal that individuals should be self-sufficient. That children obtain emotional independence at an early age probably results from a sudden maternal rejection after great initial indulgence. Thereafter, technological skills, social autonomy, and economic independence are achieved in rapid succession so that, for example, an individual at the age of nine is beyond the authority of anyone in the village. Individualism in these various areas creates an atomized society, the belief in self-sufficiency having widely ramifying implications.

Another related ideal concerns competition. Competition is ruled out because it could lead to social complementation, to the dominance of some persons by others, to a breakdown of egalitarianism and self reliance. Bateson's concept of symmetrical social differentiation (1935: 181) is descriptive first of the egalitarianism and lack of authority differences between socially mature persons and between any two social groups and, secondly, of the

expected self-sufficiency, individualism, and the resulting social fragmentation or atomism among Paliyans.

(b) *Kin classification*

Kinship terminology, kinship behaviour, pronominal classes, and the usage of personal names provide an explicit statement of the principles underlying the social structure.

Sondakārar, or relatives, can include the half dozen nearest kin, all inhabitants of several intermarrying villages or, more usually, those close kin that make up the traditional band discussed above.

Two sets of kinship terms are used in two contexts — culture contact and intracommunal (Gardner 1965; for a comparable case, see Turnbull 1965). Systematic interference occurs chiefly when contact terms are used in the intracommunal context. The two sets of terminology constitute modified Hawaiian and Iroquoian systems, the Iroquoian terminology being on the model of the Dravidian kinship of neighbouring Tamil plainsmen.

Intracommunally, the criteria of kin classification are (1) sex, (2) generation relative to ego, and (3) affinity (with spouses, with affines in positions analogous to spouses, and with the latter's children skewed one generation — husband and his relatives or equivalents up, and wife and her relatives or equivalents down).

There are two distinct sets of behavioral expectations paralleling the two sets of terms for the various kin classes. Within the community, kinship status is usually explained in terms of strict generational equality. There are no lateral biases since these would be an affront. Even intergenerational status differences are played down. The one special area concerns the wife's brother and her father, of the (intracommunally) terminologically distinct affine group, who are the prescribed vine-rope watchers during traditionally dangerous honey gathering expeditions down the cliffs. Neither is a rival for ego's wife in a society where competition must not be expressed openly and where authority is symbolized in terms of sexuality. Apart from the special, 'safe,' affinal relationships, kinship roles are defined bilaterally and generationally.

In the culture contact sphere, a different set of ideals is voiced. For example, during recently borrowed and prestigious rites of passage there have developed special roles for cross relatives (such as mother's brother), having ritual prerogatives and for cross-cousin or cross-niece being one's prescribed marriage partner. When it comes to actual behaviour, if outsiders are absent, mother's brothers are ignored in ceremonies, without comment or protest. And, in the case of marriage partners, while 22 per cent of marriages are as prescribed in the contact sphere, another 19 per cent actually take place with parallel-cousins and -nieces — classificatory 'incest.'

Pronouns in the context of contact distinguish sex and relative age. Intracommunally, sex is not necessarily distinguished, and it is absolute age (child versus socially mature) not relative age that is indicated. Respect, as they put it, must be shown toward all who are mature. Similarly, the respect barring a South Indian woman from uttering her husband's name is reciprocal among Paliyans; the husband, too, must avoid using his wife's name.

The Paliyan conceptual model agrees with the model derived from this analysis. What would be called symmetric roles (seen as generational egalitarianism, sexual egalitarianism, and non-recognition of relative age as a status differentiator) and avoidance of overt aggression are products of what the Paliyans would call avoidance of disrespect. Furthermore, Paliyans are conscious of the sort of egalitarianism that can be expressed through bilateral kin terminology.

(c) Social control

Purely social mechanisms for solving problems in Paliyan non-cooperative, non-competitive culture are not obvious. The basic cultural premises preclude either superordinately administered social controls or joint group-administered controls. There are, however, six main control devices. (1) The ideal for the Paliyan is to eliminate all overt aggression or disrespect. They call this their rule above all other rules, and they expect self restraint. (2) When friction does arise, a mature individual, known as the headman (*nāttāme*), attempts to alleviate the situation by joking with and soothing the antagonists. A village may have one or two such persons, or none at all. (3) If conflict is still not suppressed or averted, the parties must separate. A child once slapped too hard by its mother was taken off and fed for the rest of the day by another woman who was not even a relative. When spouses separate at the first quarrel the result will be a series of marriages. A village argument led to half the village going off to the forest for two weeks. Out of discretion, Paliyans have avoided outsiders, most of whom they regard as exploitative and aggressive. This mechanism, separation to avert conflict, has led to the Tamil stereotype of tribals: shy vis-à-vis the world and unstable and promiscuous in their own community. (4) Fear of sorcery is another deterrent to disrespect. (5) In serious crises gods are called. They possess and speak through the living. Authoritative decisions are achieved and reassurance is found without interfering with purely human patterns of interpersonal symmetry and independence. These gods are also thought of as sources of emotional support for the ill, who characteristically express dependence on the gods and request protection. (6) Incest, theft, and murder are expected to draw direct supernatural punishment in the form of an accident or illness.

(d) Catharsis

As might be anticipated, catharsis attends the two major areas of tension in Paliyan society—where the rules (a) against overt aggression, and (b) for the maintenance of interpersonal symmetry and self-sufficiency conflict with the needs of individuals and society.

The Paliyans have an active fantasy sphere. They particularly enjoy the violent aspect of Tamil films, and they dream of power over others. Tensions are relieved through make-believe fights or blows.

Children and elders imitate the dancing of the gods while bystanders laugh. Even worship of the deities is parodied, with men standing in as the gods. When they do possess men, the gods are cursed and mocked; commands are put to them in a manner disrespectful to a person—sufficiently so to

require separation. This is done as if to negate both the authority of the gods and the reliance placed upon them during crises.

(e) Social structure and band composition

To summarize the description of social structure, four points can be made:

First, Paliyan bands are composed of one or more clusters of closely (usually primarily) related nuclear families.

Second, the kin groups, kin classification, and kinship behaviour are based upon a bilateral definition of roles.

Third, with the exception of the role assumed by the headman between symmetrical social components in friction, the band could be regarded as an aggregate of equal and independent family (or individual) units without political organization or corporate functions.

Fourth, the instability of band and family membership is due mainly to the technique of separation as a means of resolving or preventing conflict. Nomadism is frequently the result of fleeing from aggressive neighbours.

THE PALIYANS AS REFUGEES

The Paliyans occupy only the lower slopes of the hills, a zone that is dry, stony, and almost unbelievably thorny in most areas. One of the problems is the leaching of the soil caused by solar action and rapid water run-off on the exposed, steep hillsides. The 1,000- to 3,000- or 4,000-foot zone is unwanted by either plains cultivators or shifting agriculturalists who occupy the better soil zone at higher altitudes. It is a refugee area in every sense, and the Paliyans are the first to describe its discomforts. While the yams and small game allow a comparatively dense population, they can be utilized most effectively by small, mobile exploitative units. The one resource that might possibly necessitate bigger aggregates and cooperative exploitative arrangements is large game. But the Paliyans, apparently, can forego this potential food resource, for they have not developed the appropriate hunting technology.¹ Although it is extremely uncomfortable, the natural environment does not make extraordinary demands on Paliyan socioeconomics. But it is clear that, given the emphasis on the more easily obtained food resources, there is pressure for small, mobile groups. With the natural environment actually necessitating the sort of self-sufficiency that Paliyans prefer, the various factors involved reinforce one another, heightening their impact on the social forms.

Megasthenes describes in the third century B.C. a culture similar to that of present South India and notes a number of tribal populations, some pygmoid, in areas where such tribals still exist (McCrindle 1877: 40-4, 81-8, 99, 114, 156, 189). What is more, shortly thereafter there was an era of exploitation of forest resources for export to Rome. The Tamil literature of this period (first to seventh centuries after Christ, if the conservative dates of Sastri (1958: 355-7) are accepted) describes with extraordinary precision Tamil and tribal cultures and provides additional grounds for supposing the early establishment of forest produce contracting (Mudaliyar 1959-60: *passim*; the evidence is summarized in Gardner 1965). All evidence points toward continuity not just in the cultural foci of Tamils but also in the culture contact profile between plains Tamils and hill tribes. This profile

is complex: today the Paliyans are regarded as ritually and physically unclean; they are without courts of appeal; they are exploited, threatened, and treated as children; their women are regarded as easy marks. It appears that the food-gathering tribals responded by establishing tangential contact to avoid aggressors, but they were not totally evasive, so as to be still able to acquire through barter certain necessities such as iron. The ideas of caste complementation and caste self-government are compatible with, and would have reinforced, the tribal preference for social pluralism.

The technological and social differences between Tamils and Paliyans, coupled with attitudes of superiority and exploitation by the former, can be summed up as intercultural pressure. Gardner (1966) examines in detail four psychological responses—repression, projection, displacement of hostility, and generalization of anxiety—to the perpetual sense of vulnerability and inferiority of chronic refugees, and considers their impact upon Paliyan culture. The Paliyans do, in fact, have an exaggerated picture of Tamil aggression, and (with their ever active fantasy) they read hostility even into the actions of their fellows. There is an overall pattern of retreat and individualism which does not leave a single aspect of Paliyan culture unaffected. The impact of the intercultural environment is hardly lessened by other practical considerations, since there are few economic pressures for cooperation.

In summary, natural and intercultural pressures affect Paliyan culture both directly and indirectly through the intervening variables of settlement pattern and psychology. These influences are as follows: (1) Intercultural environment presumably dictated natural environment when the Paliyans sought refuge in the inhospitable zone. (2) Both aspects of environment, but especially the natural, affect settlement pattern and group formation as previously described. (3) Residential instability, essentially neolocal or bilocal residence, and small group size, in the absence of countervailing forces, are conducive to bilateral descent. (4) Intercultural environment suggests retreat and evasion, but when contact with aggressors is inevitable, because of the marked power differences, compliance if not subservience is required.

Tentatively, it may be supposed that some causal connection exists between environmental pressure and the three characteristic features of Paliyan social structure. In particular, natural pressure results primarily in bilateralism, and intercultural pressure results primarily in Paliyan individualism, which is expressed as aggression-avoidance and interpersonal symmetry. However, cross-cultural data alone can validate these generalizations, a task undertaken in the following section.

RECURRENCE OF PALIYAN STRUCTURAL FEATURES

Out of a sample of twenty-seven gathering cultures, representing the world's main food-gathering culture areas and some subareas, seventeen cultures are either identical to the Paliyans in the three structural features or are the polar opposite in all three. The ten remaining cultures represent four of the six other possible combinations of these features (for example, lineal, non-aggressive, symmetrical). All twenty-seven cultures are listed and classified in Appendix I.

The Mala Pandaram, Kadar, Semang, !Kung, Western Shoshoni, and Southern Paiute most closely resemble the Paliyans; the Mbuti are also probably included in this group. (1) All are bilateral. (2) Their cultures indicate symmetric structuring of interpersonal relations rather than authority ranking. They avoid headmanship or attention, expect even the aged and infirm to be self-sufficient, separate in order to avert or resolve conflicts, and tolerate a wide range of behaviour (even incest) because of the limited possibilities for social control in a system precluding cooperation or competition. Where data are available it seems that children are given the same training as Paliyans to achieve independence. (3) These same cultures all stress aggression-avoidance, and this may, as is the case with the !Kung, be phrased as their primary social rule (Marshall 1959: 360-1; 1960: 327, 336).

The Coastal Alaskan Eskimos, Murngin, Walbiri, Vettuvan, Mala Vedan, Vedda, Kaska (a), Tlingit, Wintun, and Ona contrast in every feature with the Paliyan group. For them, warfare, leadership, ranked authority, and the placing of the group above the individual are characteristic. This is true also of lineality, which is not just a descent rule but the focal model in the formation of groups and the governing of behaviour within and among these groups.

The Yanadi, Siriono, Saulteaux, Kaska (b), Yahgan, Chenchu, Caribou Eskimo, Polar Eskimo, Northern Shoshoni, and Andamaners fall as clearly into four other structural types showing certain combinations of the diagnostic features (*see* Appendix I).

It is not that I am considering parallels and contrasts but rather it is the closeness of the parallels and the completeness of the contrasts that I wish to emphasize.

If pressure, change, and no pressure in both natural and intercultural environments (natural pressure judged in terms of settlement pattern) are considered, then there are nine possible stable and transitional total environmental situations. For example, the cultures from one group of our sample, including the Chenchu, Caribou Eskimo, Polar Eskimo, and Northern Shoshoni can be shown to have been subject to transition in their environmental situations. For each there have been equalizing factors which relieved intercultural pressure. As anticipated, there is a perfect fit between environmental type and culture type. Bilateralism is associated with natural pressure and aggression-avoidance and symmetry with intercultural pressure. Cross-cultural data tentatively support the proposed relationship between various aspects of environment and the characteristic features of Paliyan culture.

When environment changes, aggression – aggression-avoidance is the feature most likely to change first since it depends on very superficial aspects of the intercultural power balance. The slowest feature to change should be bilateralism–lineality because of the complexity of this structural feature. It is therefore possible to set up sequences of expected culture change given specified environmental changes. There are nine cases of environmental change in our sample, and in every case the culture exhibits the expected transitional form (Gardner 1965: 132-6).

It is even possible to show why certain combinations of features are infrequent or absent from our sample, but that is going beyond the scope and purpose of this paper.

CONCLUSIONS

Unstable residence, concern with mythical enemies, interpersonal atomism, sequential marriage, abandonment of the aged, 'toleration' of incest, lack of leadership, bilateralism, and so on, can be seen as aspects of a recurring cultural type. They no longer have to be and, indeed, should not be explained in isolation.

Part culture (Kroeber 1963), thin culture, and degenerate culture (Lévi-Strauss 1961) are inadequate labels for the non-formalized, individualized way of life of many peoples such as the Paliyans. This is a purely human adaptation to certain recurring environmental conditions. Behaviour in such a system is planned and predictable and if survival is the ultimate criterion by which to judge adaptation or viability, then the behaviour is most successful.

Although much has been written about a South and Southeast Asian hunting-gathering cultural stratum (Ehrenfels 1952; Fürer-Haimendorf 1943), most of the parallels can be disqualified as possibly convergent by combining the present ecological argument with Sapir's criteria (1949) for interpreting distributions. Cross-cultural analysis validates the logical argument that Paliyan social structure is largely the result of environmental pressures.

Goodenough's hypothesis that social flexibility due to natural environmental instability is the causal factor in Lakalai and Lapp bilateralism (1962) is given validity. However, there are numerous difficulties with the treatment by other writers of bilateral social systems, particularly those like the Paliyan system. Sahlin's use of bilateral, aggression-avoiding, symmetric hunting and gathering cultures as a baseline from which to triangulate on early cultural society (1959) is fallacious—eight of the twelve cultures that he used for this purpose probably have their present shape as a result of refugee status. In another recent study, Service has made an incorrect association between intercultural pressure and bilateralism (1962). The groups of his sample are either under combined intercultural and natural pressure or under neither, hiding the true association of bilateralism with natural pressure. His conclusion that lineality (or emphasis on unilocality) was likely to have been widespread in early hunting societies is not far-fetched. I am rejecting his assumption, however, that there are only two main alternative hunting and gathering culture types. A larger sample than Service's quickly shows that a number of societies have the flexibility and individualism of his composite organization without being bilateral, and a number are bilateral without exhibiting the whole complex of associated features of which he speaks. Furthermore, because in the past there were many different sorts of environmental types (including the possibility of intercultural pressure), gross cultural differences even in the palaeolithic are expected. The advantage which the hunting Ona had over the shell-fishing Yahgan represents a difference that could have obtained millenia ago.

NOTE

1. In this connection, there is a long list of edible plants and small game which the Paliyans regard simply as unpalatable. There is no indication that they ever have to resort to their use, although this possibility cannot be entirely discounted.

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APPENDIX I

A typology of hunters and gatherers in terms of their resemblance or divergence from the Paliyans in three cultural features.

Bilateral, Aggression-avoiding, Symmetric (BNS):

Paliyans
Mala Pandaram
Kadar
Semang
!Kung
Western Shoshoni, Southern Paiute
Mbuti

Bilateral, Aggression-avoiding, Hierarchic (BNH):

Yahgan (transitional from BAH to BNS)

Bilateral, Aggressive, Symmetric (BAS):

Caribou Eskimo (transitional from BNS to BAH)
Chenchu (transitional from BNS to BAH)
Polar Eskimo (transitional from BNS to LAH)
N. Shoshoni (transitional from BNS to LAH)

Bilateral, Aggressive, Hierarchic (BAH):

Andamaners

Lineal, Aggression-avoiding, Symmetric (LNS):

Siriono
Saulteaux
Kaska (b) (Honigmann 1949)
Yanadi (recently transitional from BNS to LNS)

Lineal, Aggression-avoiding, Hierarchic (LNH):

not represented in sample (transitional from LAH to LNS)

Lineal, Aggressive, Symmetric (LAS):

not represented in sample (transitional from LNS to LAH)

Lineal, Aggressive, Hierarchic (LAH):

Coastal Alaskan Eskimos
Murngin
Walbiri
Mala Vedan
Kaska (a) (Honigmann 1954)
Tlingit
Wintun
Ona
Vettuvan

APPENDIX II

Bilaterality of primary bonds between conjugal pairs

By using a modification of Helm's statistical demonstration of bilaterality, through analysing the nature of bonds between primarily related and co-resident conjugal pairs (Helm 1965), the figures below pertaining to Paliyan bonds were obtained. One major change from Helm's procedure is necessitated by the frequency of serial marriage and the high number of step-parent relationships. Instead of looking for son-parents versus daughter-parents bonds, the various component relationships are computed. Three examples are given, and the following coding is used:

P indicates a parent-child bond

S indicates a sibling bond

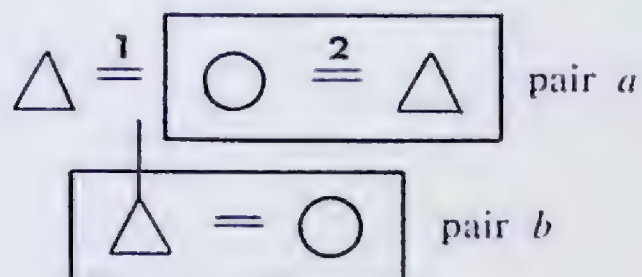
m indicates a bond between two primarily related males

f indicates a bond between two primarily related females

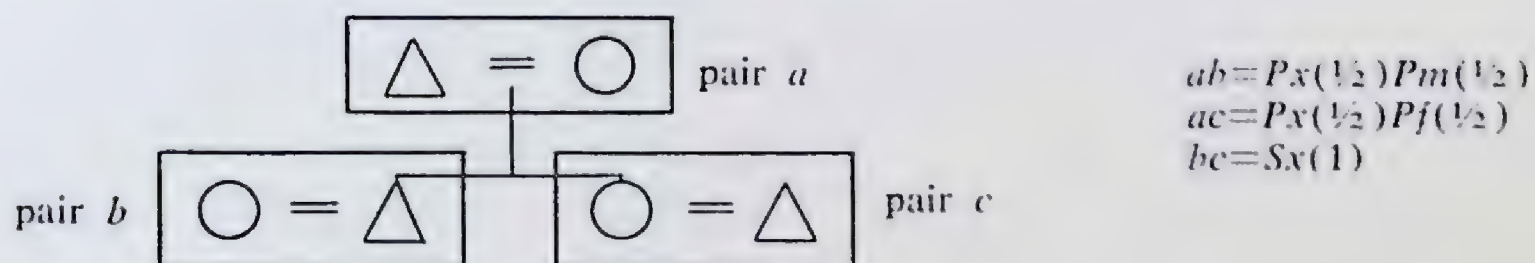
x indicates a bond between two primarily related persons of opposite sex; parenthetic numbers indicate the weight given to component relationships of the bonds.

Example 1. The bond between pairs *a* and *b* is a cross-sex parental relationship:

$$ab = Px(1).$$

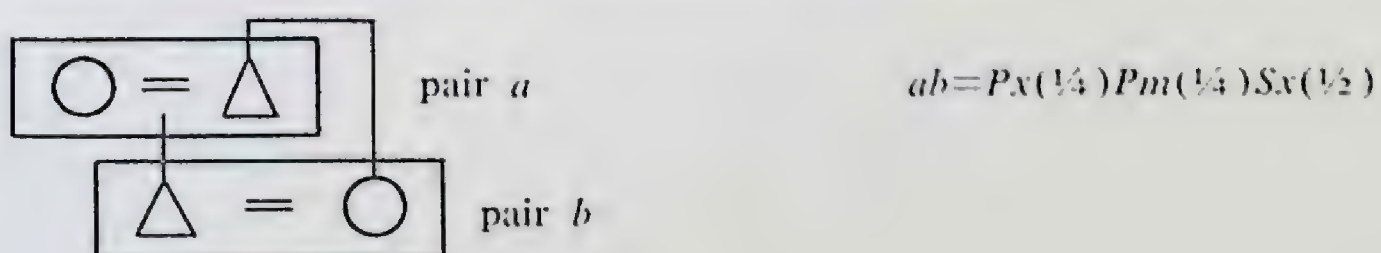


Example 2. The bonds between pairs *a*, *b*, and *c* are as follows:



$$\begin{aligned} ab &= Px(\frac{1}{2})Pm(\frac{1}{2}) \\ ac &= Px(\frac{1}{2})Pf(\frac{1}{2}) \\ bc &= Sx(1) \end{aligned}$$

Example 3. There are three component relationships to bond *ab*:



$$ab = Px(\frac{1}{4})Pm(\frac{1}{4})Sx(\frac{1}{2})$$

Tabulations of the primary bonds between conjugal pairs in five Paliyan bands

Paliyan band	Primary bonds between conjugal pairs												Total of primary bonds			
	Number						Per cent									
	Siblings			Parent-child			Total uncorrected			Total corrected						
	<i>m</i>	<i>x</i>	<i>f</i>	<i>m</i>	<i>x</i>	<i>f</i>	<i>m</i>	<i>x</i>	<i>f</i>	<i>m</i>	<i>f</i>					
A	4½	13	2½	1	5½	2½	19	64	17	51	49	20	9	22	231	29
B	3	3	2	3	5½	1½	33	47	19	57	43	8	10	16	120	18
C	—	2½	1	¼	1¼	1½	11	54	36	38	63	3½	3½	7	21	7
D	1	2	—	2	1	—	50	50	—	75	25	3	3	4	6	6
E	—	—	—	½	1	½	25	50	25	50	50	—	2	3	3	2
Totals	8½	20½	5½	7¼	14¼	6	25	56	19	53	47	34½	27½	52	381	62

If the cross-sex relationships are divided equally between male and female, then virilocal or uxori-local tendencies should be apparent from the resulting contrast between male and female emphasis in residence. Bands C and D vary from the nearly 50/50 male/female emphasis in the other three bands. However, these two bands are adjacent and related, and there is much movement back and forth between them. If C and D are taken jointly, then the bilateral structuring of Paliyan bands shows up clearly.

	Emphasis	
	Male %	Female %
A	51	49
B	57	43
C and D	55	45
E	50	50
Totals . . .	53	47

DISCUSSION

DAMAS: I am prejudiced against some of your findings. One of the difficulties with attempting world-wide comparative studies is that the ethnography is not uniformly good.

GARDNER: I am aware of this problem, but I do not know how one can get a statistically significant sample using only the best sources on food collectors.

HELM: When you speak of aggression versus non-aggression, are you considering intersocietal or intrasocietal aggression? I do not think that the two need accompany one another.

GARDNER: I find societies where there is neither, and others where there are both. By my definition of aggression-avoidance, neither internal nor external aggression can occur. Either sort of aggression would put a group in the aggressive category. I do not assume that the two sorts of aggression have to go together, but in my sample they are usually associated.

HELM: In some societies there might be extreme controls on intrasocietal aggression, but there may be at the same time considerable extrasocietal aggression.

GARDNER: People like the Australian aborigines and others who have had no powerful neighbours have elaborate and formalized patterns of aggression. Pockets of people who have been exposed to more powerful neighbours are avoiders of all forms of overt aggression. In the case of the Paliyans, there is an expression that if somebody strikes you, you turn the other cheek. They do not believe that one should return a blow even under extreme provocation, whether dealing with a co-band member or a total stranger.

HELM: It occurs to me that the only people who are not aggressive externally are those who have already been whipped. I am thinking of the Dogribs and the Slaveys. The latter group are called the Slaveys because the Chipewyan defeated them. The Dogribs, on the other hand, defeated the Chipewyan. Both groups have tremendous control over interpersonal hostilities. The people show aggressiveness only when they are drunk. The internal organization of the two tribes is identical. They are people with the same controls, with the same constraint in showing any kind of interpersonal aggression, and yet their culture history is quite distinctive in so far as the criterion you have given is concerned.

GARDNER: I am discussing long-term situations, not just individual defeats.

WILLIAMS: Could you give an example of the internal values that lead to non-aggression?

GARDNER: The Paliyans quite simply value autonomy for the individual, and they fear breaches of autonomy. For this reason, Paliyans fear outsiders whom they perceive as aggressive. Outsiders drink alcohol, while Paliyans are afraid to. Outsiders have volatile natures and are always chopping off each other's heads, according to the Paliyan. This is not all Paliyan fantasy; the Tamil people really do treat Paliyans in a heavy-handed way except in the religious context (that is, tribal people live much as religious hermits and even high caste Indians will take water from them). Outside the religious context the Paliyans are whipped, shouted at, treated like children, and not infrequently killed. When they are employed as labourers, they are paid half the wage of their fellow workers. This situation does relate to the very rigid hierarchy of traditional Indian society. However, I find equivalent forms of intercultural pressure to be common elsewhere. Wherever there are non-aggressive societies there seem to be aggressive or powerful neighbours who have been present for at least several centuries, and the association found for the Paliyans is upheld.

LEACOCK: I wonder if this situation is related to acculturation or culture contact rather than being an intrinsic quality of band society.

GARDNER: That is really my main point. I agree with Service that composite bands have certain features that result from acculturation and that societies may be almost formless as a result of this contact. People who have not been subjected to this sort of pressure have much more definite patterns of competition and cooperation, of aggression, and of social complementation. They lack the social fluidity caused by the extremes of individual autonomy. I diverge from Service on one major point, however, because my sample shows non-lineality (including non-unilocality) to be an independent feature, not related to intercultural pressure and not necessarily an attribute of the fluid band.

McKENNAN: I am concerned with the three factors which you have given. Two of them seem to me to be structural features, and the third is a matter of values or ethos.

GARDNER: I follow Firth to a large extent in my definition of social structure. I make a distinction between social organization and social structure. Social organization is the sum of actual situationally variable patterns of interpersonal interaction of the group, and social structure is the body of implicit schemata which underlies social behaviour or interaction of the group. I conceive of social structure as being a part of culture, so what I consider to be structural features may be defined a little more broadly than is usual, but I think there is a precedent. However, I can see that not everyone would agree with this terminology.

McKENNAN: I am intrigued by the approach but somewhat puzzled as well. What bothers me about your matrix is the use of bilateralism and symmetry, and aggression or non-aggression. Is it not possible that a number of factors could be used in this context?

GARDNER: I analysed my Paliyan data in these terms without thinking of cross-cultural implications. Only after returning from the field did I become concerned with comparative data. I saw that the three features that were central to Paliyan society were central in other societies as well, and on those grounds alone I took them as valid bases for comparison. I am not saying that these are always necessarily important or unimportant in every society, but in the societies with which I am dealing they seem to be the relevant features. In fact, frequently they are the important features of the societies in the eyes of the people themselves. I have some faith in the psychological validity of my conceptualization of Paliyan society; this is the way they understand their own society; these are the features which they feel are most important.

McKENNAN: I wonder whether they are all independent variables?

MARSHALL (to Gardner): In your paper you mention the toleration of incest by a number of people, including the Bushmen. I do not think that it would be accepted by the !Kung. They would think a man was mad if he practised incest.

DUNNING: Do we have toleration of incest in any society?

GARDNER: I can give an example from the Paliyan of a young man who had sexual relations with his mother until the time when she was pregnant. The stepfather then got annoyed and left the house for two weeks. At the end of that time his anger cooled, he came back and lived happily in the same house. Shortly thereafter the youngster, having found employment on a plantation, moved to another village. The whole family including the stepfather moved off with him. The question is not so much what people tolerate openly as what they tolerate by taking no action. You may call it grudging toleration if you wish.

MARSHALL: Such situations may not be unusual in Paliyan society, but they are unusual in general.

BICCHIERI: What I do not understand is the impossibility of sexual rivalry, if a particular man has a casual relationship in hunting and other matters with his father-in-law and brother-in-law.

GARDNER: There is some feeling that you cannot have relations with your own mother or your own sister or daughter — that is the way they put it. I suppose there could still be rivalry over the wives of wife's father and wife's brother, but at least neither of these male in-laws should want to take your wife. You should not have to fear them; you could trust them to hold the rope as you descend a cliff for honey. Even if they did become rivals, incest is something that is supposed to be taken care of by the deities. As long as the gods are taking care of the situations, who cares? The Paliyans do not ridicule each other. Many of the usual devices for control which can

be used informally just are not present. You are expected to restrain yourself, and if you cannot, the headman comes to cool you off. If he is unable to cool you off, you separate. In larger matters the gods take over and that is the only way they can be dealt with.

BICCHIERI: Are these mechanisms effective?

GARDNER: They are effective. In the case of the young man who had relations with his mother, the boy got a boil on his heel which was said to have been caused by his incestuous relationship. It lasted for two months, and he could not work during that period.

BICCHIERI: Among the Chirichaua Apache, incest is related to witchcraft unless the family can deal directly with it.

GARDNER: This might be the same mechanism. I brought up the matter of incest to indicate the inability to have social controls in a society that is organized in terms of marked individual autonomy.

HELM: There is no way to focus authority, so it is acted upon beyond the family.

GARDNER: Yes, and I have seen people trying to be challenged in order to get their opinions voiced.

SLOBODIN: In other words you are saying that this is their means of survival, and your reference to turning the other cheek fits very well.

A CULTURAL ECOLOGICAL COMPARATIVE STUDY OF THREE AFRICAN FORAGING SOCIETIES:*

The Net-hunting BaMbuti, the Southern Kalahari Bushmen, and the Central East Africa Hadzapi

By M. G. BICCHIERI

RÉSUMÉ

Trois groupes, à la fois chasseurs et cueilleurs, font ici l'objet d'une analyse comparative établie en fonction de leur civilisation et de leur écologie: le groupe BaMbuti, presque exclusivement chasseur, le groupe des Boschi-mans du Kalahari du Sud et celui des Hadzapis de l'Afrique centrale de l'Est. Le but de cette analyse, qui prend pour point de départ la *Théorie des modifications de la civilisation* du professeur J. H. Steward (1955), est: 1) de vérifier les covariations écologiques et culturelles qui se présentent dans les sociétés à forme simple; 2) d'élaborer, s'il se peut, une généralisation descriptive qui précise les postulats de Steward au sujet des «bandes de chasseurs organisés par rapport à la lignée paternelle». Les généralisations relèvent de trois classes de constantes appartenant aux trois civilisations et qui correspondent à trois variables: 1) le niveau de complexité culturelle; 2) le mode d'exploitation considéré en corrélation avec l'interaction de la technologie et de l'habitat; 3) le caractère restrictif ou, au contraire, facultatif de l'habitat. Donnons un exemple de ces résultats: il semble plus logique d'employer, pour désigner ces groupes, l'expression «orientés vers la prédominance masculine» que celle d'«organisés en fonction de la lignée paternelle»; il semble aussi préférable d'identifier les groupes par rapport à un territoire (qui est une entité plus permanente que l'agglomération humaine qui s'y identifie) plutôt que de les identifier à une bande de composition immuable possédant un territoire hérité en lignée paternelle.

Resulting from intensive analysis of three foraging societies, this paper investigates the component features of band organization in the context of Prof. J. H. Steward's characterization of the *patrilineal hunting-band*. It employs cultural ecological analysis, a method which is particularly effective in the study of simple societies. The corpus of this paper is contained in the accompanying chart in which all the relevant elements of the analysis are juxtaposed and the interpretation is summarized. Although the chart embodies all the comparative data, because of its schematic nature I will present a few pages of commentary. This commentary is subdivided into two sections: (1) methodology and chart construction, and (2) evaluative interpretation.

*The body of this paper was derived from the writer's M.A. thesis presented at the University of Minnesota as partial fulfilment of the Master Degree requirements and written during the tenure of an N.I.H. training fellowship.

METHODOLOGY AND CHART CONSTRUCTION

(Comparative Chart in pocket at end of Bulletin)

Following O. Lewis's classification of comparative studies (1955: 264-5), this analysis can be characterized as a cross-cultural, synchronic, controlled, formal-functional, typological comparison, geographically restricted to one continent. The comparison is cross-cultural because it involves three distinct African societies; it is synchronic in that it is based on ethnographic material which does not deal with change over time. The constant level of sociocultural complexity and the three contrasting habitats are the chosen control factors, controls which permit the observation and evaluation of the variations for which the ecological correlations are sought. Finally, since the investigative categories, which do not separate form and function, are evaluated on the basis of quality rather than quantity, the study is typological rather than statistical.

An examination of the techniques used in this comparative study, as expressed in the chart, reveals the following: The two control elements, incorporated under the title 'Basic Referents,' are (1) the constant level of the sociocultural complexity, and (2) the independent variable—the habitat. By keeping the level of complexity fixed at the low level congruent with foragers, and by varying the habitat, the antecedents for an analysis of co-variation are obtained.

To prevent distortion resulting from the complexity of factors involved, a number of corrective measures have been introduced. First, the environments and the independent variables have been selected on the basis of their marked contrast to each other in order to make their cultural concomitants readily detectable. Second, the environmental factors operate on a constant, minimal level of cultural complexity, a level at which the interrelationship among cultural features is not complicated by excessive structuralization. Last are the two techniques of comparative methodology: (1) the fine breakdown of the ethnographic material into juxtaposable categories, and (2) the systematic, logically consistent derivation of progressively finer statements of co-variation.

The sample consists of (1) the Southern Kalahari Desert Bushmen, (2) the Hadzapi of Central East Africa, and (3) the net-hunting BaMbuti of the Ituri Forest. They all share a constant level of cultural complexity based on different, but equally simple, types of multifamily association and elementary technology. More specifically, the relevance of the sample is based on one fundamental aspect, the distinctiveness of the habitat. Taken in order, the three societies' contrasting environments—desert, scrub-forest, tropical-forest—present increasing permissiveness in providing subsistence. The simple cultural level and sharp environmental contrast combine to make the subject matter well suited for this investigation.

The ethnographic material is broken down into detailed cultural and non-cultural categories. The headings of these categories appear in column 1 at the extreme left of the chart (all columns are marked at the bottom of the table). The crucial non-cultural category, the *habitat*, precedes the cultural categories which start with and follow from *Man-to-Habitat*. To facilitate comparison, the main categories have been subdivided, as the content of the

material dictated, into finer headings. These finer subdivisions express details which are relevant to cultural ecological analysis. No separation of form and function was involved in the breakdown of the data.

After the completion of columns 2, 3, and 4 with information from the ethnographies, the systematic inspection of these juxtaposed items was undertaken. Similarities and differences were noted and expressed as *Comparative Generalizations* in column 5. The last operation, presented in column 6 as *Derived Regularities*, concerns the translation of the comparative generalizations into cross-cultural regularities.

In compiling column 6, the very essence of the derived regularities suggested a further subdivision into three classes of cross-cultural regularities corresponding to three variables: (1) the *level of cultural complexity*, (2) the *exploitative pattern* concomitant to the interaction of technology and habitat, and (3) the *restrictive-versus-permissive* aspect of the habitat. These classes represent the maximal analytical extension generated by the sample under this particular mode of scrutiny. The last column, 7, *Steward's Postulations*, sums up J. H. Steward's cultural ecological assumptions which are of consequence in evaluating the patrilineal hunting band. It is arranged to match the categories of column 1.

EVALUATIVE INTERPRETATION

Basically this section of the paper is devoted to the comparison of columns 6 and 7, that is, to the contrast between the basic assumptions and derived regularities of this study and Steward's postulations. This comparison is used as the initial step in developing a growing understanding of the characteristics of foraging societies — not as a negative criticism of Prof. Steward's work. Mainly restricted to the *patrilineal hunting band*, the discussion contains a limited number of pertinent remarks on cultural ecological relationships.

Some general observations can be made. The use of minimal level socio-cultural complexity and simple technology is agreed upon in both analyses. This technology can be called *bow-spear-club* and *dibble-basket* because resources, other than game, are often the crucial staple. In this study, habitat is an independent variable; Steward employs habitat, with regard to his cultural typology, as a constant when stating that limited and scattered resources, plus small non-migratory bands of game, are the constant ecological factors that produce a constant cultural type (Steward 1955: 123-4). The nature of the sample has already been noted. In the course of this analysis, a number of features, such as types of social controls and variations from individual to cooperative subsistence techniques, are considered as integral parts of the culture core rather than secondary features.

The following more detailed evaluation of the three classes of regularities suggests that male dominance results in a basic *virio-orientation* among foraging societies. Virio-orientation means that when no specific factors, such as a particular mode of exploitation, influence a particular pattern of authority, residence, or descent, the general orientation as a result of male dominance is toward the male. However, this is only a predisposition and can be significantly altered by varying environmental conditions which favour different patterns of exploitation. Steward sees male dominance as a derivative of the importance of the hunting-male's economic role (viz.: determined by

economic dependence on small non-migratory bands of game exploited by means of a simple bow-spear-club technology) and as an explanation of the patrilineality of the hunting band.

Furthermore, the menarche is indigenously stressed by formal education. More elaborate puberty rituals for both sexes seem related to culture contact. It is not clear whether this fact confirms the hypothesis that simple societies stress biological changes rather than social changes (van Gennep 1960: 69), or whether, independent of the first show of blood, the stress on female puberty is a social fact motivated by the necessity of focusing the neophyte woman's attention on her forthcoming responsibilities as a wife and as a mother (Honigsmann 1959: 513).

In this present study, the band, the maximal autonomous unit, is identified with a specific area which it exploits for its survival. This territory is a more permanent entity than the composition of humans identifying with it. Variations in the nature of the territory due to ecological differences or seasonal changes influence the nature of the group existing within it. According to Steward, the band's composition does not alter because the subsistence problem is the same in all cases. He sees the association as fixedly patrilineal and the tendency to be otherwise rare, temporary, and disappearing, giving way to the ideal patrilineal form (Steward 1955: 124). According to Steward, the territory is owned by the band's leader and inherited through the patrilineal descent line (1955: 125).

The next regularity states that decisions preponderantly concern subsistence. This is a very important point when considering the relationship of culture core to secondary features, and therefore, of ecology to culture. It is consistent with the definition of culture core that a group whose decisions are mostly concerned with subsistence problems will also exhibit patterns of behaviour, which are mainly culture core activities.

Maintaining social order rests heavily on the informal controls provided by ritual and recreational activities. By virtue of the kind of activities undertaken and the fact that subsistence forms the basis of regulatory decisions, this last regularity also suggests the prevalence of culture core behaviour patterns among foraging peoples. Ritual and recreational activities are generally secondary, in Steward's opinion. In contrast with patrilineality, patrilocality, exogamy, land ownership, and lineage composition, which constitute elements of a cross-culturally recurrent culture core, religion and other similar aspects of culture are local variables of an independent nature (Steward 1955: 122).

The next analytical derivation states that a mythical creator god is present who is an anthropomorphized aspect of a significant natural element. This statement is not meant to be revealing or provokingly new but rather to reiterate the veridicality of the proposition of the close circuit habitat-culture relationship. This relationship is further emphasized at the bottom of class (1) by the proposition that the value orientation derived from man's relation to nature, to time, and to action is constant, not as the result of the nature of the particular habitat but because of the common level of cultural complexity resulting from the lack of advanced technology. The value orientation, analysed in F. Kluckhohn's terms (1950: 376-93), is surmised rather than observed — yet it is significant to note how the outcome of this category fits

into the general scheme and validates the implication of the chosen constant.

Class (2) concerns the actual relationship of habitat to culture. Regularities emerging in this class derive from the hypothesis that, as the exploitative technique in response to a specific habitat varies from individualistic to cooperative, a series of cultural manifestations are shaped as its concomitant variables. A shared viri-orientation among foraging groups is significantly altered by ecologically created local differences. The resulting variation is a de-emphasis of sexual division of labour in association with cooperative patterns of exploitation. This analysis does not support the suggestion of patrilineality, patrilocality, and patriarchalism as universal constants subject to non-significant local variations. Instead, it suggests that the ideal viri-oriented pattern is maintained when no pressures for a choice of social structures is brought to bear by the habitat. It is significant that whenever a decision must be made, the subsistence factor is generally favoured. D. Forde strengthens this suggestion when he notes that for foraging peoples, "systematic organization through rule governing the residence of families, the affiliation of individuals to groups and succession to status or economic rights in accordance with descent is lacking because the ties latent in parentage and siblinghood are prevented by the ecological situation from formalized expression in the social organization" (Forde 1959: 70).

With the cooperative pattern of exploitation, a more rigid demographic relationship among foragers is found. The size of the band is less flexible when, as in the instance of the net-hunting BaMbuti, individuals must behave as component parts of a productive, collective whole. With the individualistic pattern of exploitation limited, increase or reduction in band membership can be managed because ecological permissiveness alone is the unknown and not the permissiveness of an integrated collective whole.

Social control, in the present foraging people context, is very much part of the culture core. The maintenance of a specific social pattern which secures the livelihood of a group is very much a part of "the constellation of features which are most closely related to the subsistence activities and economic arrangements" (Steward 1955: 37). As the exploitative pattern varies from individualistic to cooperative, the focus of consensual decision is transferred from the family to the band. In addition, social control punishes more heavily crimes threatening the cohesion of the group, such as a breach of the incest tabu, adultery, theft, and crimes endangering the livelihood of the group (uncooperativeness in subsistence tasks, for example). Sanctions which, in individualistic-oriented groups such as the Hadzapi, stress compensation are replaced in cooperatively oriented groups like the net-hunting BaMbuti by sanctions which stress socioeconomic isolation. Furthermore, patri-jural authority is replaced in cooperatively oriented groups by a more consensual authority held by mature individuals who display conciliatory abilities. Steward accounts for the slightness and informality of leadership by saying that leadership is a kinship-derived status located in the lineage head and, as such, is slight and informal (1955: 126). The suggestion that "the shaman . . . controls many collective activities, and he is feared and respected for his supernatural power, which often gives him more influence than the other leaders" (1955: 126) finds no support in my sample. The attributes of the magicoreligious practitioner observed in class (3) are

dependent variables of the restrictiveness or permissiveness of the habitat.

As the exploitative pattern varies from individualistic to cooperative, the nature and stress of the ritual behaviour vary from isolated to agglomerated and from individualistic performance (Bushmen) to total group participation (BaMbuti). Religious rituals are vital social integrators, and, at the level of cultural complexity under consideration here, ritual activities, even though informal, are directly concerned with the maintenance of a specific social order. The social order reflects the habitat-created economic arrangement.¹ Consequently, the significant aspects of ritual activities are not easily altered by cultural diffusion. This proposition finds support in C. Turnbull's observations on the BaMbuti's cultural integrity.

The analysis of the modality of relationship reinforces the suggestion that the cultural patterns of most foragers are dependent variables of ecological features. The modality of relationship changes from individualistic to collateral as the economic pattern varies from individualistic to cooperative. To a certain extent, the above observation agrees with E. R. Service's stated relationship between egocentric and sociocentric statuses. These statuses correspond to individual and group consciousness respectively (Service 1962: 12).

The regularities postulated in the third and last class depend upon the degree of economic permissiveness of the habitat. More precisely, it is postulated that as the habitat varies from restrictive to permissive in enabling the foragers to gain their livelihood, several concomitant variations are noted.

First, opportunistic nomadism is replaced by more stable migration with cyclical re-use of camp-sites. Second, the attitude toward non-producing members of the group, such as the very young or the very old, becomes more accommodating. Third, a shift occurs in the locus of informal authority from people possessing ascribed status to people exhibiting achieved status, from status derived from hereditary transmission of scarce commodities to status secured through personal attributes. This point is of great cultural ecological significance because it is derived from the empirically observed correlation between inherited authority and scarcity of important resources. Within my sample, the Bushmen display the greatest degree of inheritance of authority. In the Kalahari desert, this legacy is connected principally to the inheritance of the very vital and very scarce commodity—permanent water. The limited supply of this crucial item cannot be jeopardized by random and uncontrolled use. The danger of misuse and loss is offset by establishing nominal individual ownership of the commodity on the condition that the nominal owner cannot refuse access to the water. It is also suggested that the commodity owns the individual rather than vice versa; in a sense it is a case of stewardship rather than ownership.

The relationship of the people to the supernatural varies from fearful to trustful as the environment varies from restrictive to permissive. A similar variation is noted in the innate predisposition, from highly mixed to good. The 'shaman' is a magicoreligious practitioner, and his presence and status among foraging people are determined by the nature of the man-to-supernatural relationship, which, in turn, is influenced by the nature of the habitat. Within the permissiveness of the Ituri forest, no proper shaman exists; the informally performed rituals are generally group performances. In the

Kalahari, where uncertainty of food supply is always latent, economic anxiety is reflected in the ambivalent attitude toward the supernatural. The Bushman's innate predisposition is highly mixed—most men and women are magicoreligious performers of good or of evil, as the situation arises.

It is well to conclude with the reminder that the observations derived from my chart refer to a sample of only three societies. Even though the derived regularities are self-expressions of the intrinsic qualities of the tabulated material rather than the offspring of a preconceived orientation, the study's effectiveness on the whole must be maximized by widening the sample and by varying the problem formulation. There seems to be a distinct need for clarification of cultural typology. It appears that in a number of cases, such as shamanism or ownership, the comparative, cross-societal analysis is hampered by the existence of a condition in which it is not at all clear whether labels or concepts are being compared. Whether band organization or openness versus closeness in peasant societies is investigated, a crucial antecedent is the clarification of referent conceptual classes which can be used cross-culturally with the reasonable certainty that general societal regularities can be formulated without bringing to life a new taxonomy. For example, groups should be compared along the continuum of differentiable modes of population control rather than within the taxonomic scope of presence versus absence of infanticide or genocide.

NOTE

1. This topic has been treated in more detail by the writer in a previous paper, "The Molimo of the BaMbuti: A study of ecological effects on ritual and social control manifestations" (1963). This study was concerned with the analysis of the "habitat—social-order—informal controls," and its theoretical referents were works by M. Gluckman (1963) and E. A. Hoebel (1954). Based on C. Turnbull 1962.

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DISCUSSION

DAMAS: How large were these groups of which you were speaking? I am concerned mainly with the size of the total populations, of the Kalahari Bushmen for instance.

BICCHIERI: My data come mostly from Schapera, which is a collected sample of Bushmen groups, mainly from the southern Kalahari, the least acculturated at the time. I do not recall their numbers.

For the BaMbuti I use Turnbull's figures, which refer mainly to the net-hunters. These people were dispersed into groups of four or five hundred.

DAMAS: What I should like to determine is this: Can you assume uniformity in all these characteristics in all the groups in the three areas? I cannot appraise for the Central Eskimo all these matters that you have charted here, but I found less uniformity in social features in that area than I had expected. The regions are quite uniform ecologically, and I cannot explain local differences of social organization in terms of their corresponding to local differences in environment or ecology. The natural subareas (to the extent that they exist) and the cultural subareas are not always congruent as they should be in situations that could be explained in a scheme like the one you have given. I am not sure that all these things that Steward, and probably yourself as well, would call cultural core (principally features of social organization) are immune to historical factors.

BICCHIERI: I do not think that Steward and myself would suggest that these core features are immune to historical factors but rather that they probably would provide more resistance to change than those features that we would consider to be secondary. I suggested in my paper that more features are culture core features in hunting and gathering societies than in more complex societies. In the more complex societies there are a larger number of secondary features than there are in the simple societies that have a more or less hand-to-mouth existence.

EGGAN: I think that the Central Eskimo situation that Damas speaks of would be more comparable to comparisons among the various Kalahari Bushmen groups and that Bicchieri's comparisons are more macroscopic.

DAMAS: Where would you be likely to place the people that I have described in terms of your scale with reference to your three groups?

BICCHIERI: I place the Copper Eskimo below the BaMbuti regarding permissiveness of environment and close to the level of the Bushmen.

GARDNER: The problem of permissiveness of environment is a rather difficult one, though, because it is very easy to think of its being difficult to exist in the desert and easy in the forest. Most people who have visited the Ituri say that subsistence is very difficult there. The food is very scarce. Regarding Australia, the desert situation might be thought of as being comparable to the Kalahari, but Meggitt's recent work points out that there are long-termed aggregations of three hundred people in central Australia. He maintains that Spencer and Gillen were quite wrong about the Arunta, and others were also wrong about them. It is my contention that you cannot

really judge permissiveness of environment very easily. In addition, you must take into account the complexity of technology. If a very good technology exists in a very poor environment, the relationship with the environment might be very good. I feel that you can make the assumption, however, that people will gather in large groups if they can. I hold that if only small population aggregates are allowed by the environment, people customarily have to live a very mobile unsettled life in which band composition is likely to change rapidly to make the best possible use of resources, and if the family is the largest unit of cooperation allowed by the ecological situation, then one can speak of severe environmental pressures. In this case, and if one can believe Meggitt, I would say that the central Australian desert does not exert great pressures on the people. I would call it a permissive environment for the Australians.

EGGAN: Europeans have not been able to live there yet. Does this mean that the Australians have a better technology?

GARDNER: That is a significant point. Perhaps one should say permissive ecological situation rather than permissive environment.

WILLIAMS: Not everyone accepts Meggitt's conclusions. As well as rejecting much of the former ethnography that has been done in the area, he bases much of his material on interviews in 1951. The data he throws out are based on 1928. The situation may have changed.

GARDNER: Meggitt thinks that the Walbiri are less acculturated now than the Arunta were when they were studied; this would be unusual but possible. We all know of enclaves of people that are not touched.

BICCHIERI: I find the Australian situation to be satisfactorily accounted for in my scheme.

GARDNER: Even though my figure of fifty (to differentiate small and large bands) may be as arbitrary as yours of sixty-four, your grading of the environments is still unclear to me. I do not understand how you can grade the environments as being permissive or non-permissive. There has to be a concrete index.

BICCHIERI: My variations were in this case based on the literature. When I followed the patterns out, I used a mean variation as to what were the people's actual situations. I used a list of factors on the basis of my relative judgment. In connection with this list, I used a scale that runs from zero to six, increasing the number according to increasing severity. I have included such items as the technology and the range of movement of game and have assigned value to each society on these bases. The measurement which I used may not satisfy in its relativity, but at least I have applied it consistently to the sample of thirty societies.

DAMAS: Did you use the criterion of population density?

BICCHIERI: I used mainly the nature of relationships with nearby people, whether it is interdictive(restrictive) or whether it is amiable(permissive).

Density was regarded as being a result rather than a factor influencing the relationship.

WILLIAMS: You state that the ideal virilocal residence situation is maintained by the pressures brought to bear by the habitat. I find with the Bihor that residence is not particularly determined by the habitat but rather by the personal choice of hunting partners.

BICCHIERI: I meant to imply that some of the reasons that are given in the literature for lineality did not seem to apply in my sample. There may be a tendency toward patrilocality in hunting and gathering bands, as Service says, because of the defence of territory which is a product of male dominance in general, but these factors alone would not have made a society patrilineal. Incidentally I do not use 'patrilineal' or 'patrilocal' for such situations, I simply said 'viri-orientation.'

The impression that I have received so far at this conference is that we have been talking much more about kinship than we should have been, that what is actually happening in these bands is that an active producer is being replaced, that there is a general tendency in a hunting group to replace hunters that have been lost to the band. I would suggest that the viri-oriented situation is one of neutrality. This might apply for 70 or 80 per cent of the residence situations in a band, but I am more interested in what happens to the other 20 to 30 per cent — the situations where there is choice available. These comprise what I call 'substitution of producers.'

With reference to the BaMbuti, all the decisions in residence that are mentioned by Turnbull seem to have been very practical pragmatic ones made at the time. There appears to have been very little that could be called ideal virilocality unless there was absolutely no need to make a decision.

GARDNER: Your argument regarding virilocality is interesting, but does it operate in groups where hunting is much less important?

BICCHIERI: In cases where the economic importance of women is greater than that of men, there might be a tendency for women to replace women.

On the other hand I think that Service also makes almost a biological statement in his hypothesis that the need for protection points toward a viri-orientation, a statement which has significance for human evolution.

GARDNER: Would not that pertain only to hunters?

BICCHIERI: Is it not also true for gatherers? It is not always true that women are the most important sex in a gathering society. In the case of the BaMbuti the men also gather food. For the Tiwi, men spend much of their time perpetuating the economic system, directing gathering activity.

DUNNING: Let me shift the discussion a bit. When you talk about viri-orientation—I should like to suggest that regarding exogamy in a situation where divorce is rare and where there are virilocal residence patterns, rules of exogamy are not needed if there is an incest tabu. Is it becoming overly explicit if you talk about exogamy? There are incest tabus and exogamy in all these groups. It seems to me that this is an automatic association.

BICCHIERI: My feeling is that in terms of incest, commands rather than prohibitions exist as in Lévi-Strauss's approach. The command is: "Go and marry outside the group." This is especially true of a small group. Exogamy would, in a sense, be a part of the same command.

MARSHALL: I did not find this to be the case among the !Kung Bushmen. The band is not the marriage or exogamic unit. Marriage is, rather, regulated by kinship. The band is made up of permanent residents who can marry within it.

BICCHIERI: When we say that people can marry within the band, that they are endogamous, we are simply saying that when someone is attached to the group who is not one of the people that you consider to be relatives, then you can marry them. But since you do not always find non-relatives within the band, you must go outside the group to marry. Another point it would be well to make here is that most of the studies of exogamy and most of the statements that have been made here have been made with respect to a particular situation at one point in time. I suggest that in a hunting and gathering society where there is a marginal subsistence situation, what you might observe at one point in time might change if there has been an epidemic or a change in the movement of game. These conditions will affect marriage practices. I think that this applies to the !Kung. A Bushman should marry someone unrelated to himself; and if that person is from outside the band, then the situation is one of band exogamy—whatever that means. With respect to incest regulations we are just saying that you marry outside your close kinship group.

HELM: It is possible to practise endogamy without the existence of a 'rule' about it. Or you can believe in it and enforce it—there are all sorts of variations in endogamy and exogamy.

Marshall has referred to persons within the !Kung band who are not kinsmen. How did they become band members?

MARSHALL: The marriage regulations prohibit marriage through first cousins. Marriage of second cousins is not really allowed, but it is not considered a great breach of the rule. As to the kind of people who could come in, those who would not be related would come in usually through the practice of bride service. During bride service a young man could bring all his family and some of their affines with him and they would be welcome. The fact that they lived together in the band does not mean that they cannot marry. At least I have that impression.

GARDNER: I do not remember the exact figures, but about 50 per cent of the Paliyan marriages are band endogamous. In the Kibbutz, the children that are brought together have the feeling that marriage within the Kibbutz would be incestuous. They have Kibbutz exogamy.

EGGAN: There is some argument about whether it is a real feeling of incest or whether it is a feeling that "we just grew up with these children and we'd like to marry someone else who is more exciting." Spiro has documented the case quite carefully and found in the early Kibbutz that people who actually grew up in them together did not marry. Children who came into the Kibbutz and those who were born in it formed two groups.

PART II:

OBSERVATIONS ON BANDS

By JULIAN H. STEWARD

RÉSUMÉ

Étant donné la grande variété des sociétés qui ont été dénommées «bandes» et l'impossibilité de déterminer des critères à priori convenant à des types variés de bandes, on insiste pour que la question soit abordée d'une façon véritablement empirique, basée sur une comparaison qui procède cas par cas et où l'importance que l'on accordera aux processus de développement soit celle que l'on accorderait à des caractéristiques diagnostiques. Toutefois, la signification méthodologique de ces processus par rapport au facteur causal est évaluée plus loin dans *Post-scriptum pour les bandes: de la taxonomie, des processus et des causes*.

THE CONCEPT OF BANDS

I suggest that minimal importance should be ascribed to a search for criteria of bands and to a construction of a typology of bands. A few years ago, a large week-long conference on plantations in the New World virtually wasted four days attempting to define plantations. It was not until the conference recognized that there were many kinds of plantations, each resulting from a special complex of cultural-historical and ecological factors that the participants began to isolate the distinguishing features and causes relating to each kind of plantation. Again, in my recent cross-cultural study of modernization, my collaborators and I recognized that a typology of contemporary native societies could not be the ultimate objective. Any typology was valuable, mainly in suggesting dynamic processes of change. In the case of bands, as in the two cases just cited, it may be far more profitable to search for those processes which have brought about the distinguishing characteristics of societies subsumed under this very broad category.

A cursory review of societies called bands suggests an extreme variety of social forms ranging from those found among hunters and gatherers to quite complex structures such as those exemplified by the camel nomads of North Africa and the near East and the mounted Mongols of Inner Asia. Any definition or typology of bands among these complex societies must be open-ended because it is impossible, and probably futile, to establish criteria which can clearly delimit the category of social structures known as bands.

I think the only useful heuristic diagnostic and delimiting criteria of bands are that these societies shall have, first, a fairly wide-ranging nomadism (which may or may not involve territoriality) and, second, permanent membership, even though the society fragments into smaller groups during certain portions of the year. These delimitations would make the concept of bands include many, though by no means all, hunters and gatherers. Among mounted nomads, such as the Arabs and Mongols, and among herders, such as many East African tribes, the extent of nomadism and the amount of

seasonal fragmentation are such that their inclusion as bands is a matter of definition.

I think the most fruitful approach to bands by any definition is to analyse them in terms of those evolutionary processes which brought them into being rather than in terms of a static typology which leaves unanswered the impossible, and perhaps futile, question of which of the many characteristics are the most important for taxonomic purposes. After all, Linnaeus's taxonomy of biological forms was but a prelude to Darwin's evolutionary interpretation. One may note similarities in social structure, but the deeper understandings come from a knowledge of the processes which brought about these structures.

STRUCTURE AND PROCESS

Societies which may be designated 'bands' do not necessarily include all simple hunters and gatherers. It seems to be a preconception of anthropology that all societies must have consisted of groups larger than the biological or nuclear family. Among the simpler hunting and gathering peoples, the nature of the social unit was determined to a very large extent by the processes of cultural ecological adaptation, that is, by the nature of social interaction required for subsistence in a given environment by means of a given technology. The social environment, as contrasted with the natural environment, is also a factor in shaping the nature of any society, but its role is minimal in most of these cases.

Causal factors also involved adaptations to the social environment, especially to comparatively large social groupings during short periods of food abundance, intergroup hostilities which require coordination of group efforts beyond what subsistence demands, performance of certain religious rites, and others.

SOME SUBSTANTIVE EXEMPLIFICATIONS

Below the sociocultural level of interfamilial cohesion that would be required to classify a society as a band were the Western Shoshoni, probably the pre-horse Ute, and many of the Northern Paiute whose ecological adaptations prohibited permanent social aggregates consisting always of the same group of families. The independence of the Shoshonean family cannot be doubted, despite Elman Service's apparent effort to classify these people as a patrilocal or patrilineal band in some kind of evolutionary scheme. The occurrence of bands, i.e., suprafamilial groups of constant membership, occurred in the Great Basin only in the few comparatively fertile areas where foods could be found within one or two day's travel from the village.

Despite the complete difference in environment and natural resources in the Chilean Archipelago, subsistence upon shellfish similarly required that the Alacaluf and Western Yahgan remain fragmented in family units except on the rare occasions when a whale was stranded on the beach.

If one conceives of a band as consisting of a number of families which associate and interact with each other on a permanent basis, one must look for different kinds of cultural-ecological adaptations. One can call certain hunting bands patrilocal or, as Owens has recently suggested, patri-

focal, although I still prefer the term 'patrilineal' owing to the interaction of a number of factors. I am sure there are variations within this category, although I have not yet seen any clarification of subdivisions of it.

Walter Taylor has recently called attention to a special kind of cultural-ecological adaptation in northern Mexico which has led him to designate these societies as "water-tethered" bands. In an environment with foods not unlike those of the Great Basin, the water holes are so far apart that clusters of families are held together by the sheer impossibility of wandering too far from their water supply.

There are probably other distinguishable kinds of bands among primitive hunters and gatherers, although there is a lack of comparative studies of their structures and cultural-ecological adaptations. I have suggested that the multi-family bands of caribou hunters in Canada may represent the result of a special adaptation. Perhaps, too, there is some comparability between the wild-rice gathering Guatô of the upper Paraguay River and the Ojibwa of the Great Lake region. Still further comparative studies will surely disclose additional kinds of societies among primitive hunters and gatherers.

In several independent instances the introduction of the horse has created a new type of society which may be designated the 'predatory' band. Two examples of this occurred respectively in the Great Basin and in Patagonia. Preconditions of the predatory band are the presence of European settlers in the area who may be raided and of feral domesticated animals which were introduced by Europeans, and the mobility provided by the horse. Among the Northern Paiute and Ute in the Great Basin, as among the Tehuelche and Puelche of Patagonia, the small social aboriginal units became grouped into larger bands to raid settlements, to hunt the escaped domesticated animals, and often to make war on other bands. These bands had fluctuating membership because families allied themselves with leaders who seemed to promise rewards. Families shifted allegiance to another leader if he seemed to be more successful. Omer Stewart's contention that bands existed in the Great Basin in aboriginal days seems to involve a common anthropological preconception that all primitive peoples must have had bands, and his evidence for these bands confuses the post-White mounted predators with the native designation of any inhabitants of a local area by the most common food eaten there.

The concept of the predatory band could be extended into far larger and more complex societies if people such as the Mongols of Inner Asia were to be included.

If organization of families in a permanent cohesive group and lack of a fixed residence are among the criteria of bands, the camel nomads of North Africa and the Near East represent bands of another kind. These people, however, were predatory on the oasis farmers only to a limited extent. Because they relied upon the oases for vegetable food, they could not afford to sack and destroy the farm populations. At this upper or open end of bands, one might conceivably include the predominantly cattle-breeding East Africans, such as the Masai and others who of necessity engaged in

some degree of nomadism. Considerations of these more developed peoples, however, involve us in the unanswerable quantitative question of what degree of nomadism and permanence of headquarters places any society in the category of bands.

METHODOLOGY

I suggest that any comparative study of bands avoids an a priori reification of the concept. Terms such as band, village, tribe, and others have long been used in anthropology, but without qualification they lack heuristic value. An empirical procedure should not start with the assumption that there must be a category of societies which can be classed as bands and then seek diagnostic characteristics by which to place them in a typology. To the contrary, it should adopt a method of case-by-case cross-cultural study wherein similarities are noted and categories are constructed on the basis of empirical fact. This method would avoid the unanswerable question of what is a band.

I further believe that analyses of whatever may be designated 'bands' will be more enlightening if they are made in terms of the processes or special combinations of processes which have brought about their evolution or transformations. In many cases, such as the patrilineal band, which is found in different parts of the world, the principal processes are basically adaptations of the society to the environment by means of its technology. In the case of the patrilineal band and other early societies where historic depth cannot readily be obtained, these adaptive processes, though inferential, are fairly convincing. In other cases, such as those wrought by acquisition of the horse, the basic evolutionary processes are clearly spelled out.

Cross-cultural studies have the advantage that hunches or hypotheses concerning the processes and resulting social forms concerning single societies may be validated or revised by extended comparisons. Such studies, moreover, help isolate the developmental processes which, I believe, in the final analysis are a primary goal of studies of cultural change and evolution. Recognition of cross-cultural similarities of a purely synchronic nature in social structure and other features is a means to this end.

In comparisons I ascribe primary importance to social structure, which includes kinship systems, marriage patterns, residence types, and cooperation in subsistence and perhaps other endeavours. Religion, world view, art forms, kinds of clothing and houses, and many other features must be adapted functionally to the basic kinds of social structure but cannot be primary criteria if viewed in terms of their formal distinguishing features.

CRITERIA OF IDENTIFICATION OF BANDS:

INTRODUCTORY REMARKS

By RICHARD SLOBODIN

RÉSUMÉ

La Conférence ne se propose pas d'aborder tous les types de groupements communautaires couramment désignés sous l'appellation de bandes. On peut sans doute dire des populations pastorales qu'elles forment des bandes, mais le sujet de la Conférence est, à proprement parler, l'évolution des cultures, ce qui écarte par définition les communautés pastorales.

Les sociétés de chasseurs-cueilleurs dont il est question ici sont caractérisées par la précarité du contrôle des ressources, par l'absence presque totale d'accumulation de biens matériels héréditaires et par la persistance de la parenté considérée comme un facteur d'organisation. Il est bon, toutefois, de noter que la parenté elle-même peut revêtir, dans ces sociétés, des aspects différents: elle peut être contractuelle, prendre la forme d'une association ou présenter simplement un caractère spontané. Il est rare que l'autorité soit exercée d'une manière officielle ou continue.

We are all disappointed that illness has prevented Professor Julian Steward from being with us. I understand that Dr. Damas had hoped Professor Steward would introduce the present discussion. Professor Steward was able, however, to send along a short paper called "Observations on Bands," which Dr. Damas has passed to me. Although it may well be felt that the most useful contribution I can make is to read you this paper, on consideration I do not feel that I should. Copies of it will be available to all participants later.

It happens that I disagree substantially with Professor Steward's observations. To read them to this meeting and refrain from comment would be to shirk a certain intellectual responsibility; on the other hand, to read them and attempt a rejoinder would, in the absence of their author, be rather fatuous.

I cannot refrain, however, from referring to at least one of Steward's comments. He opens his remarks by suggesting that we waste as little time as possible in searching for criteria of bands and in constructing a typology of bands; in effect, he suggests that we omit the effort proposed for today's session. He concludes his plea thus:

I think the most fruitful approach to bands by any definition is to analyze them in terms of those evolutionary processes which brought them into being rather than in terms of a static typology which leaves unanswered the impossible and perhaps futile question of which of many characteristics are most important for taxonomic purposes. After all, Linnaeus' taxonomy of biological forms was but a prelude to Darwin's evolutionary interpretation. One may note similarities in social structure, but the deeper understandings come from knowledge of the processes which brought about those structures (*See p. 188*).

It is difficult to see how it is possible to examine or identify processes which may have brought something or a class of things into being before deciding what the things are, or what the class is. Steward's illustration from the history of biology would seem to refute rather than to confirm his point, for the development of taxonomy and systematics was an essential prerequisite to the development of an adequate theory of organic evolution. A. R. Wallace was a great systematist and Darwin himself made important contributions in this field. One of the major weaknesses of pre-Darwinian theories of organic evolution was inadequacy in taxonomy, and as a result anti-evolutionists were able to point out absurdities in suggested chains of development and in posited processes.¹

This procedural requirement has been clearly set forth by Julian Steward:

Three requirements for formulating cultural regularities may be stated in a rough and preliminary way as follows:

- (1) *There must be a typology of cultures, patterns, and institutions. . .*
(Steward 1955: 180). [Italics in the original.]

As Dr. Leacock has remarked, in studying societal evolution anthropologists are working from both ends: moving up from primate behaviour and down(or back) through ethnohistory. This leaves an enormous gap. What, in fact, can study of the peoples we have been discussing tell us about (a) primordial human social life, or (b) general principles of organization at a simple or, as Steward implies, a low level of sociocultural integration? Each speaker and discussant have been well aware that we have been dealing with peoples, all of whom, with the exception of the Central Eskimo, have long been affected by contact with one or more complex and powerful civilizations. Even Dr. Damas was describing, not primordial man, but man in highly evolved societies. It must be, then, that we all cherish the belief, or at least the hope, that in the study of contemporary or recently existing societies—at least the nonliterate ones, and perhaps the literate as well—it may be possible to discern principles of social organization, and that if these are valid principles, Lyell's Doctrine of Uniformity applies, making possible extrapolation backward in time. Coupled with this hope is the consideration that in the simplest economies the possibilities for drastic change are relatively slight, occurring in human history only in peculiar circumstances. As Betty Meggers has commented in reply to the culture-historical argument that all observed primitive societies have just as long a history as the complex societies, "the question is not . . . whether there has been time for change, but rather what kind of change could have taken place" (1960: 311).²

One result, perhaps, of the social-evolutionary interest which motivated this conference is that up to this point no pastoral nomads have been considered, or even mentioned. The term 'band' has frequently been applied to groupings of pastoralists—for example, by Murdock (1949: 80) and by Steward in the remarks which he has forwarded. We, however, have been talking about peoples in the Upper and perhaps some in the Middle Status of Savagery, rather than those in Middle Barbarism—so long as I am going to be evolutionary, I may as well be Morganatic—and this is just as well, for if nomadism itself were to be used as a criterion for band level

of integration, quite dismaying and irrelevant complications would result. Moreover, one feature that characterizes the lives of all the hunting-gathering people we have been discussing is the tenuousness of their control of the ecosystem. Despite the extraordinary systems of floral and faunal knowledge in the cultures of most or all of these societies, as well as ingenuities in technology, the combinations of habitat-pressure and (or) technological limitations are such that these peoples have little or no vested interest in the accumulation of commodities. They have property, of course—even the Siriono have their hammocks and bows—and also capital: tools, weapons, and above all, knowledge and skills. For all practical purposes it is the latter alone that is heritable; there is almost no tangible heritable property. This is almost as true of the eastern Kutchin, who, despite periodic famine, were relatively far above the sheer subsistence level; it is also true of the !King Bushmen, the Siriono, or the hill people of India who have been discussed at this conference.³

It seems to me that this is a very important criterion of level, or range of levels, of social-economic complexity, because from the presence or absence of tangible heritable property ensue many social consequences: in marital choice and prescription; in authority; in settlement pattern; and, generally speaking, in the relative tightness or looseness of cultural and social structuring. The implications for type of ethos are so great that the range of differences in ethos among the kinds of hunting-gathering peoples discussed here seems rather slight when compared with the contrast between these and the Northwest Coast peoples, or with such marginal pastoralists as the Reindeer Chukchi.

In a latter-day continuation of Morgan and Maine's contrast between kin-based and territorial- or contractual-based societies, some recent theorists have characterized band organization as strictly a matter of kinship, lacking, or almost lacking, any quality of sodality. Surely since the work of Lowie, it is impossible to make a black-and-white dichotomy between kin and non-kin relationships. I should like to suggest that kinship be considered as obtaining on two levels. First, there is deliberately acted-out kinship of the kind that most of the previous speakers, and, indeed, most ethnographers have discussed. This, I suggest, is in considerable measure a form of sodality, perhaps especially so among peoples at the band level of sociocultural integration. Second, there is kinship as the basic condition of full humanity, of 'tribal' membership in what I take to be the sense suggested by Dr. Helm (1965: 362 and *passim*). It is likely that everyone in the band members' universe of discourse is kin in this sense, except the alien, who is not fully human, as tribe members all have the potentiality for kinship in the first or active sense. The contractual aspect of kinship in band society may be seen in analyses of Australian kinship from Radcliffe-Brown through Elkin and Warner (and in fact may be discerned in the classic earlier ethnographies); it is beautifully illustrated in Ronald Berndt's discussion of law and order in aboriginal Australia (1965).

The relationship of kinship *in posse* (my second type, above) to kinship *in esse* (my first) may be noted in an incident and a type of event, each commonplace enough, among the Kutchin. At Chalkyitsik, Alaska, a

Kutchin village, I mentioned Arctic Red River, a Kutchin village in Canada. The two settlements are separated by three hundred air-miles and by the Cordilleras; it happened that no one in the company had heard of Arctic Red River. After some initial scepticism, my interlocutors were happy to be convinced that the Arctic Red River people were Kutchin: "So we have relatives over there! We can go there!" Thus the universe of potential kin is extended.

It sometimes happened in the past, and it still does happen on rare occasions, that an encounter takes place between parties of Kutchin who are quite unacquainted. The oldest members of each party proceed to compare and to trace genealogies until a connection is made; whereupon if they are women or elderly men, they embrace, weeping together for all the dead who lie between them. Thus potential kinship is converted into active kinship.

The comment may be made that these qualities of kinship are not peculiar to band organization. They are not entirely so, but they are more relevant criteria for the band level of organization than for other levels, because of the relatively undifferentiated structure of both society and culture at the band level and also because of the strict limitation, by ecological constraints, of possibilities for action. From the individual's viewpoint, there are few specialities (Linton 1936: 272ff) and also few of what Linton calls "alternatives" (loc. cit.). To continue with Linton's terminology for the moment, there remain "Individual Peculiarities." Even in a small and homogeneous community of humans, there is a noticeable range of these; it was to this, I think, that Dr. Leacock was referring, as well as to our tendency at times to over-sociologize our explanations of behaviour, when she cited an informant who asked why he might not go hunting with a man just because he liked the fellow.⁴ Where there are few specialities and few alternatives, the contractual and voluntary aspects of active kinship are more important than in more complex societies. The search for determinants of choice, a kind of approach which is evident in much of recent social and cultural anthropology, is particularly important for the study of band society.

It is perhaps a truism that at the band level of integration and subsistence, formal authority and continuing leadership are rare. Surely one reason that this is the case is that people at this level cannot afford, cannot tolerate, having their freedom of movement and choice — that is, such freedom as the ecology permits — restricted for long by the only kind of strong authority that can emerge, i.e., the camp bully. The looseness of band structuring, its *ad hoc*, heuristic, opportunistic nature, is not concomitant with strong, continuing, and pervasive authority. The converse, that is, the increase in the means and effectiveness of social control and of formal authority with the accumulation of property, is well illustrated in the early development of village life, succinctly summarized by Braidwood in his little book, *Prehistoric Men* (1963: 150-1).

Before concluding, I wish to say that I agree fully with those who question the notion that (an imputed) virilocality in the simplest band type stems from the differential advantage it provides men (in contrast to women), of hunting in familiar territory. When first I read this — I think in Murdock — I knew it did not apply to the eastern Kutchin, but I thought that the reason might

be that they were not all that primitive. I am glad to see my doubts corroborated by other evidence and on general grounds. In fact, it appears that those who proposed the idea simply were not well acquainted with hunting-gathering peoples.

Our task this morning is, in Dr. Damas's words, "to . . . clarify just what we mean by bands before we describe how to analyse their composition, how to classify them according to evidence of marriage practices, or what their significance is in terms of an evolutionary sequence." My aim has been to initiate discussion by pointing out some of the assumptions underlying this attempt, and also some of the general characteristics of the kind of human existence, which is the subject of this conference.

NOTES

1. The very difficulties experienced by pre-Darwinian taxonomists in the definition of species led directly to the development of the theory of evolution, as pointed out by Wallace in the opening pages of *Darwinism* (1889) and by many subsequent historians of the theory (e.g., Gilmour 1940; Smith 1958). On the analogy with the history of biological science, Steward may be correct in suggesting that efforts of the kind attempted here are "but a prelude" to the working out of satisfactory social evolutionary interpretation. If such proves to be the case, this conference will have been remarkably worthwhile.
2. There are difficulties with Meggers' point, but they need not be discussed here.
3. Not considered here are (1) land-tenure by the maximal group and (2) incorporeal property which may be inherited, and which certainly is property in a real sense (Hallowell 1955: 243, citing Lowie and Hoebel). Neither of these involves accumulation or the effects of either partible or impartible inheritance, as do agricultural lands and herds.
4. Linton does not fully elucidate what he means by 'peculiarities,' but the term is used here in a literal sense to include not only variations in personality constellation but variations in situation and life-space from person to person and, for a given person, from time to time. It would include the consideration mentioned by Berndt: "No one person has exactly the same kin alignments as the next. . . . Also, roles change according to age and seniority — expanding or retracting, as the case may be. Kinship sentiments vary with both genealogical and spatial propinquity. . . . And in practice, other than kinship considerations may intervene" (1965: 169-70).

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DISCUSSION

DELIMITATION OF BAND SOCIETIES

SLOBODIN: As you see I have not attempted here to present my views on characteristics of bands. I am wondering what magic number of characteristics is involved?

We might also take up the question posed by Dr. Steward whether we really do associate a band with the simple hunting and gathering level by definition. We do not mean bands of pastoral nomads, and we do not mean predatory bands. If we state what by definition we mean by bands, we might obviate much unnecessary discussion.

DAMAS: I think that we all (the participants) have worked with hunting and gathering groups and that we would perhaps go beyond our competence as well as our limitations in time if we expanded the discussion beyond these groups.

HELM: Perhaps we can say that we are limiting ourselves to groups which are not food producers, groups which display some sort of shifting settlement pattern. If we do not specify shifting settlement pattern, discussion will have to include groups like the Northwest Coast Indians who are also not food producers but do not shift settlement and are not usually considered to be bands.

EGGAN: By your definition we could not really exclude the Northwest Coast Indians because they did shift settlement from summer to winter. They had villages which were relatively permanent over a number of generations, but so did some Eskimo groups.

DAMAS: Yes, for example the coastal North Alaska Eskimo lived perhaps ten months of the year in permanent villages to which they returned each autumn, but in the summer they roamed in small hunting groups. I am not sure that I would class such people with our nomadic bands. I would say, rather arbitrarily, that people who live in villages with permanent structures which they inhabit for half the year or more should probably not be dealt with here.

EGGAN: We probably should not discuss such groups as these because, for example, the Northwest Coast people represent one crystallization of a set of processes which started with a simpler situation. Ultimately we have to take these groups into consideration, but I do not think that we have time in this conference to analyse Northwest Coast culture and society as a final product of this process.

GARDNER: Using the ethnographic atlas, I surveyed the 91 gatherers, hunters, and fishers who were rated six or higher in one dominant subsistence area, that is, people who depend 56 per cent or more upon gathering, hunting, or fishing. Hunters have an 0.75 Yule coefficient of association with a fully nomadic or migratory settlement pattern; gatherers have an 0.73 association with seminomadism; and fishers have an 0.96 association with sedentary settlement. These are statistically satisfactory associations obtained from

an adequately large sample. There are also differences in group size; the gatherers are mostly below 50 and the hunters above 50. And there are further differences in family organization. One wonders whether the old economic criteria are as irrelevant as is often asserted to modern process-oriented typology. There is the further question, too, of whether excluding sedentary peoples arbitrarily from our consideration will give us a useful sample of non-food producers. Is the sedentary band a logical impossibility?

THE PROBLEM OF AGGREGATION

McKENNAN: For our discussion to proceed in an integrated fashion we will have to agree upon a basic set of concepts. Most basic, of course, is the question of just what types of groups we will call bands. I would argue, for instance, that with regard to the Kutchin it is legitimate to talk about tribes (if we exclude the criteria of political organization). Within the Chandalar Kutchin, there are divisions which I would call bands. Slobodin, however, working with the same people, prefers to use the term 'band' for the entire Peel River group and then analyses the Peel River Kutchin in terms of constituent groups. I think that it would be helpful to agree upon a definition of 'band,' if it is possible, before we go further.

SLOBODIN: This brings up the question of the quality of aggregation which has been noted in some of the papers. It does happen that there was a total assemblage among the Peel River Kutchin, but I do not believe that the quality of assemblage should be the touchstone regarding bands. I raise the question whether actual co-residence of any type is needed.

HELM: This is an important question. Are we going to consider only groups that aggregate at some time 'bands,' or are we going to include others? I agree with Slobodin, that this quality of periodic total assemblage should not be a crucial criterion of bands.

DAMAS: That criterion works so well in the case of the Central Eskimo that I expect to find it in operation elsewhere. (To Helm) Did your aboriginal Déné bands all aggregate in this way?

HELM: They probably did not all unite in any one season. The majority of them are likely to have gathered at one of the big fisheries, but I do not think that they all came together every year.

EGGAN: If these examples are typical, there are probably two main subdivisions among bands: those that normally aggregate at some time in the year and those that conceive of themselves belonging to a 'band' but aggregate only in part. A question that we may ask is whether there are only a few exceptions to this criterion of aggregation or whether each of the two subtypes has a number of examples.

LEACOCK: If we think of bands as being minimal viable interdependent units, then aggregation is not necessarily an essential feature. Service indicates that a group does not have to come together as a whole to be interdependent and to function as a band.

EGGAN (to Leacock): I am interested in the size component to this minimal viable interdependent unit of which you speak. With relation to this concept, would you include Julian Steward's central Nevada "family plus" groups as bands?

LEACOCK: This is our question.

DAMAS: I think that Service felt that the Shoshoni did relate to larger than family entities.

EGGAN: I would agree in part with Service. Steward makes a distinction between family level and band level, but his family level is always a 'family plus' level or a multiple family level. There are larger aggregations, but the same families are not always present, so that there is a conceptual unit which every year is actualized to a certain extent, but its constituents are not the same each year. Each family may camp one-quarter of a mile away from the others. The people do not actually come into close house-to-house contact, but they are within shouting distance of one another in the same valley. The neighbouring families join their caches, but I think that it is the marginal subsistence situation that keeps settlement spread to the extent that I have described, even at the time of closest clustering.

ROGERS: The present-day settlement pattern in Northern Ontario, which may have some antiquity, is an interesting parallel to the Shoshoni situation. In the northern half of the region the villages are clumped tightly together; the houses are 10 to 20 feet apart. In the southern part of the region, houses are generally separated by one-quarter or one-half mile, for instance along a lake shore. There is basically the same economy in both parts of the region, so I see no ecological reason for this. Witchcraft might be a factor, but I cannot be sure.

EGGAN: In groups such as the Shoshoni, the clustering occurs every year. Although one family may be at one point one year and at another the next, there are always these aggregations for a period during each year. If we think of such people in terms of bodies of uniform personnel, they do not comprise groups; if we think of them in terms of being communities, they do.

NAME IDENTIFICATION OF BANDS AND TERRITORIALITY

SLOBODIN: Another criterion of bands might be identification by name with a given territory that is occupied or exploited. Does this always occur?

BICCHIERI: So far as I know it does, but perhaps it would be more useful to think of the peoples we are treating here as having a pattern of banding that relates to certain residence situations rather than to a specific territory. In other words, when bands are considered in terms of residence pattern, their residence should not be described as belonging to a specific locality but rather to a large area within which they could be localized at any one point, according to exigencies of life. They would refer to their residence as the larger area, within which they are mobile. One can be either from Alaska or Anchorage.

McCLELLAN: Do you think that this is what actually happens? I would say that the people that I know (Atna, Tagish, Tutchone) do name a definite locality and that, while they are dimly aware of the larger territory, they rarely designate it.

EGGAN: This range of identification by name might be complementary to the range of political centralization which refers to an important feature of band organization—that of authority. There are bands which are relatively autonomous. There are bands which bud into equivalent bands. There are also the bands of the Great Plains which had varying degrees of political centralization. If you asked a Plains Indian what he called himself, he might say Cheyenne, Arapaho, or Gros Ventre; or he might say "I am the member of such and such a band" within the larger context. The Plains groups are interesting because for two-thirds of the year they were identical to the northern bands in that each one was independent; each band operated in its general territory along a stream and had no political structure except that there was a head man. Then the bands came together in the summer, and each became a part of the camp circle and was under the authority of a group of chiefs for that period. Soldier societies acted as camp police. Punishments for breaches of peace were confiscation of property, whippings, or even publicly sanctioned executions. In two parts of the year there were two different levels of political control. During part of the season the band was independent, and during the rest of the year it was merely a unit in the larger grouping. It was a situation that is transitional to groups that operate entirely on a tribal chiefdom level.

ROGERS: Of course there is the matter of levels of organization that can be developed further. Among the Boreal Forest hunting groups there are gatherings of the full-size bands which correspond to the Cheyenne winter band, except that in the north gatherings occur in summer in connection with fishing. These groups in turn break up into smaller units for the winter.

EGGAN: Then there is the question of a bond of language within regions. In a paper in the "American Anthropologist" (1965: 675-90), Owens hypothesizes that since the Indians of Baja California were characterized by a confused combination of dialects and marriage patterns, this was perhaps a widespread phenomenon. In the northern Paiute area, there are villages whose members intermarry with the Shoshoni and are bilingual. This is true of most of the groups in the Great Basin. People marry across language boundaries. Is the common dialect bond equivalent to the band bond in the northern groups? (To Slobodin) In the case of the Peel River Kutchin, are other groups, not Kutchin, who happen to speak a common dialect treated as relatives or just as strangers who happen to speak like them? If they could not find relationships, would they be potential enemies?

SLOBODIN: The common language is important. They would, however, seek through the elders some sort of genealogical linkage, and they would find it. Of course my fellow Athapaskanists would say that the languages or dialects form a gradient.

McCLELLAN: I think that the common dialects of the Southern Tutchone hold a group of bands together but that they do not pose any special problem with respect to marrying out of the group. Their moiety organization can be surmounted.

McKENNAN: The boundary between Southern Tutchone and Han would be more difficult to determine with regard to marriage and interaction than the boundaries between Upper Tanana and Lower Tanana or Kluane and Selkirk. When members of these groups would meet, they would attempt to determine clan relationships. This was complicated by the fact that the names for the clans often differed from group to group.

DUNNING: Regarding the Kutchin, is there a marriage boundary or interaction boundary that is created by the language boundary? Is there a point at which you can say that they do not see one another as interaction partners?

SLOBODIN: On the contrary, occupying the river drainage is a group of bands divided into regional dialects, but whose inhabitants think of themselves as one people. There is one possible exception, however: the Han-Kutchin consider themselves to be different, and therefore in that case the social and ethnic boundaries and the linguistic boundary coincide. Most marriages take place within the band, the next largest number with the neighbouring band, and so on. Even now the incidence of marriage outside the Kutchin speakers drops off very sharply.

GARDNER: Are we not arguing for a consistency that may not always be present? In some areas of the world there are 2,000-square-mile areas with very little and gradual change from one culture to another, no real boundaries. We can go to another part of the world and encounter very sharp boundaries. Some of these occur where there are different environmental situations which sharply segment the area. Both situations apply to language or any other aspect of culture. In many places there are firm lines — in many places the connubium is the same as a political unit, which is the same as the linguistic unit, which is the same as the cultural unit. In other places there is a continuum in any or all of these things. I wonder if we are not trying to force a conformity to our materials which does not exist? Perhaps we cannot generalize on this level.

HELM: We cannot generalize until we have examined situations like this, case by case; this is, of course, what we are trying to do here. All these features—linguistic boundaries, cultural boundaries, and marriage universe boundaries—are worthy of being examined in this context because they are important in the self-identification of people.

RESOURCES AND TERRITORIALITY

LEACOCK: Another aspect of territoriality is that of the role of resources in determining settlement and movement patterns. Gardner has earlier referred to variations in mobility in societies depending heavily on hunting, as compared to gathering and(or) fishing.

MARSHALL: The basis of the band of the !Kung Bushmen was the ownership of resources. That is what the band was—those people who had rights to the property.

HELM: There is an ecological contrast between the Kalahari and the northern groups that I have been talking about. The Bushman's resources are discrete, scattered, and few. In the North there are fish everywhere and, consequently, there is not the close identification or ownership of a tract. The Déné do not own their fishing lakes.

WILLIAMS: Let me suggest something else, that there is a difference between Canada in general and the rest of the world in these respects. It has been suggested that it is a situation of disorganization that leads to the lack of territoriality; that is, with respect to delimited territory of a band which really controls resources, as in the case of Australians where the band territory was really an economic phenomenon sufficient to provision all the people.

DUNNING: I do not quite understand your statement that the Canadian situation is different. Does it have to do with Canadian political identification and the government contributing resources?

WILLIAMS: It has been suggested by Service that among these groups there is an amorphous movement of people; that is, where you can go, who you can live with, or whether or not you have to observe a territorial boundary. He has suggested that this situation is the result of epidemic disease.

McCLELLAN: Both situations, that of "ownership" of territory and that of a certain electiveness, occur for the Tutchone. They have sib or moiety "owned" lands which are exploited under the direction of those who own "them," and they also have some land which they say is "free." Thus they will say that a certain sib or moiety "owns" this side of the lake, and another "owns" the other side. This means that local representatives of the sib or moiety have the right to put up caribou fences or to trap. But moose can be hunted anywhere, by anyone who happens to be travelling through.

HELM: The Bushman situation reminds me more of the Northwest Coast where sib groups have fishing rights. Other people can fish there only after permission is given. But again this is a highly localized resource.

McCLELLAN (to Slobodin): Regarding the Kutchin, do you not have the feeling that a certain drainage is to be exploited by the local group? On the other hand you have referred to the free mountain country.

SLOBODIN: The heads of the rivers are a kind of no-man's land, and, as I mentioned in my talk, the people comprising the bands met at the heads of the drainage area. The river drainages are definitely held by these bands—that is their territory.

HELM: I think that in the MacKenzie region several bands might join in a hunt outside the territory of one or the other, if one or the other band is without caribou. I am not saying, however, that they did not recognize

territories, but they would not for instance say "we are here because this is our land and we hold it."

LEACOCK: Nor would they say that in Labrador. For instance, the Barren Ground people call themselves Barren Ground People and they generally live in the same area. However, if members of another band are starving, they may enter the area to hunt. It is a highly fluid situation in response to animal migrations.

DUNNING: One contrast between the situation of the Bushmen and these northern groups is that in the North there is plenty of space—there is not a scarcity of territory.

HELM: In this regard Damas and I maintain that movement into territory of other bands usually takes the form of individual families who enter the new band and attach themselves through primary kin links which can usually be found.

WILLIAMS: Is there some agreement, then, that there is a sense of territoriality in all our groups but that there is quite a lot of difference in permeability of the boundaries?

HELM: Or how affiliation is made through boundaries. In the case of the Déné, for instance, every Dogrib in each band can probably find a primary link in another Dogrib band, though this may not apply elsewhere in the world.

LEACOCK: Among the northern Algonkian, kin are not remembered very long. Also the people do not try to find a kinship basis for friendship.

McCLELLAN: If they did have kinship bonds in other bands, would language be used as a binding force?

EGGAN: This point is interesting because in the middle Columbia River area task groups are organized on a multi-tribal, multi-language basis. That is, Salish and Sahaptin elements often joined in buffalo hunts, berrypicking, and trading. Sometimes these groups included men and women, sometimes only women, but they were made up in a way that ignores kinship and language boundaries—at least in historic times. Does this happen in the North? It may be a local specialization in overriding some kinds of boundaries in favour of advantages that trade and communal activities could provide.

HELM: I should make a distinction. I said that usually one does not join a new band unless a primary tie is there to be exploited. That statement should, perhaps, be modified. Visitors can stay a season without close connections, but, as Damas says, if they stay with the band for a period, marriage ties usually build up and join them into the unit.

McKENNAN: Another factor involved is the state of affairs between two groups at a given point in time. Eskimo have stayed in Chandalar Kutchin country and vice versa. However that type of situation did not occur everywhere in Alaska, for I am sure that the Upper Tanana did not go into Kluane territory when the subsistence situation was unfavourable. There is also the additional problem of the boundaries being very indistinct in some areas.

McFEAT: Someone made the generalization earlier that groups tend to expand into as large as possible numbers, and the fact that they are territorial bears a relationship to this expansion into large groups. The point I should like to make is that there is territoriality in relationship to sparse resources but that it would be hard for groups to be territorial without this necessity.

EGGAN: The distribution of the resources must be important. Regarding the oasis quality of Bushman resources, it seems to me that the outer boundaries are less relevant than ownership of a particular spot.

McFEAT: I think that there is a difference between groups merely occupying territories and those that defend their boundaries habitually. This is part of the discussion between Speck and Leacock regarding whether the northern Algonkians are territorial.

LEACOCK: The disagreement is over individual ownership of hunting territories, not over a general group feeling about the territory inhabited. Actually the feeling may be that they belong to the territory rather than that the territory belongs to them. There are also many social ties extending beyond the territory.

McFEAT: Still the argument with Cooper as well is that of the family hunting territory being a very general phenomenon.

DUNNING: Is the issue with the land in northern Canada not that there has to be flexibility aside from birth and death and that there is variability in man and in animal populations? If the Ojibwa rely on certain kinds of animals, they will move over a fairly wide range of territory to get them. It will mean that over the years some territory will be vacant. The oasis type of ownership of territory is unknown in that area. A large amount of land is required in a man's lifetime.

EGGAN: However, a lake or a stream valley may be the focus of the group's interest because that is where the resources are, and the barren uplands, which are often the boundaries, are perhaps relatively unimportant. I think it is important to consider how evenly the resources are distributed. Historically, when a trading post is established a dramatic effect occurs. The resource base is changed.

WILLIAMS: Should we consider that Eurocanadian institutions have made possible this cross-tribal contact, since people from different bands meet at the trading posts and can more frequently move into other territories during trips to the post?

SLOBODIN: It works both ways because the advent of the fur trade frequently decreases tension.

HELM: This territoriality in terms of exclusion of like peoples across a boundary is an animal ecological problem. I can conceive of groups following highly migratory animals in exploiting an area, but at times they do not, and the territoriality does not operate exclusive of resources but seems rather to be related to questions of band assemblage that have a tendency to relate

to kinship factors. In other words, the fact that there is not the territoriality that under many conditions might ordinarily be effected can be explained in terms of the dynamics involved here.

SLOBODIN: This may have to do with the fact that through kinship there may have been relationships outside the immediate group, and this in turn relates to the problem of the viability of human groups.

BIOLOGICAL VIABILITY OF BANDS

EGGAN (to Leacock): What is the smallest unit for the Montagnais-Naskapi? Strong talked of one band, the Davis Inlet group, as having 30 to 36 members and another, the Barren Ground, as having 56.

LEACOCK: That would be the minimal size of the winter 'bands,' several of which would come together in the summer around the trading post. The summer aggregates became larger and larger over time. For instance, the Seven Islands 'band' now consists of hundreds of people.

The question that I raise here, however, is whether it is *economic* or *biological* viability that should concern us. In other words, might not a small group be, in theory, *biologically* viable but none the less not really viable? When threatened with starvation, a group of 20 would have to turn to others for help.

DUNNING: However, a group that small could not be biologically viable unless there were no incest regulations.

EGGAN: I can cite the case of the Lacandon of Yucatan as an example of small biologically viable and independent groups. At times some Lacandon bands diminished to 12 to 15 members. At such times marriage would take place between half-siblings and between uncles and nieces. After the population reached that low level, incest regulations were broken. They do not like to do this, but they had to, otherwise the group would have died out. An alternate way of arranging marriage within small groups is that of brother-sister exchange as practised in central Nevada. This form of marriage has as a corollary cross-cousin marriage which tended to develop if the same members continued to form the band. With that form of marriage, a minimum of 12 to 15 people is all that is needed in order to furnish enough marriage partners for one generation to intermarry. This is about the size of Steward's 'family plus' groups. So it is possible for isolated groups of 12 to 15 to marry within a group for a generation, but after that, it is difficult to predict what happens. For permanent survival I suspect that 25 to 30 is more commonly a minimum.

LEACOCK: This is the crucial point, that is, that the 15 is only a temporary minimum. In times of stress there has to be some dependence on other groups, which is probably strengthened through marriage ties.

WILLIAMS: From what you say, these groups would have to have economic dependence on other groups despite whatever biological viability there is.

LEACOCK: They would simply die out if there were no contacts in time of stress.

EGGAN: Regarding the Lacandon band, other Lacandon bands are known by ethnographers but not always by the individual band members. In a couple of generations if the band were fortunate in survival of children, it might build up to 15 or 20 persons and then continue to expand on its own.

HELM: Actually, then, this little group of Lacandon that you are calling a band is the society as well—the whole thing.

EGGAN: They are groups that have tried to remain viable by staying away from civilization, groups which have been reduced in population through disease and accidents so that there are no eligible mates left in terms of incest regulations.

McKENNAN: I would tend to agree with the lower limits that have been suggested for a biologically viable band. There are two such units in the areas where I have worked, one Upper Tanana band of 16 and a Chandalar Kutchin band of about the same size. Family composition was similar in these two aggregations, and with preferential cross-cousin marriage they could have conceivably continued. However, both of these bands have access to trading posts so that perhaps this minimum figure would not have applied under prehistoric conditions. In addition, so far as viability is concerned, the Upper Tanana group had ceased to be a group, when I last visited the area in 1962.

HELM: I think that somewhat larger groups must be considered if we are concerned with entities which could continue for hundreds of years.

GARDNER: Regarding the Paliyan, there are small groups of 18 or 20 which seem to persist for an extended period. Another nearby group, the Malapandaran, break up into subfamily units because of the economic pressures—parents live in one valley and adolescent children in another, for periods of three or four months. They travel for miles to find a woman to marry, and if that fails quasi-incest and even brother-sister marriage occurs.

EGGAN: We have been talking about the smallest possible size for a band, but this raises the question of the pattern of the society and the sort of network of interaction that exists. I think that, in theory, individual families living separately could comprise a viable network of interaction.

DUNNING: I was thinking of the marriage isolate which is quite different from the minimal economic unit for a continuing society.

McKENNAN: Groups like these survive for a period of years, but they are part of a larger aggregate which needs a term, perhaps Honigmann's 'macroband' or even 'tribe' if the political implications of this latter term are ignored.

EGGAN (to Leacock): I suspect from Speck's data on family hunting territory that there was normally a minimum of two families on a particular trap-line. Would it be possible for them to operate as independent units in Montagnais society and remain there all year?

LEACOCK: In the aboriginal situation the tent group of 15 to 20 was the basic unit. The question is, how many of these tent groups gathered together during the summer. I do not know whether we will ever be able to say, because in the first records the groups gathering around the Jesuit Missions and the trading posts are doubtless larger than at earlier times.

DUNNING: This is the minimal viable marriage isolate. What is the minimal size of this total complex?

LEACOCK: Marriages can take place anywhere in Labrador. Bands might go to one post one summer and to another the next summer, and encounter different bands.

DUNNING: My argument here is that the reason they have to go down to another post the next year is that the suitable marriage partner of the right sex and age is not available either in the home band or in one that they might meet in a given year. They need a large group. Did Gardner not mention three thousand as the marriage universe? It is a very large group that is needed to form a marriage pool that is effective over the years.

BICCHIERI: Did Gardner not say that marriage pools of the Paliyan are spread out in a ridge-line fashion? That is, the centre group and the ones along the edges on either side form the marriage universe, though marriage usually takes place between contiguous groups. Under these conditions it is not merely the contiguous groups that are connected, as indicated by Dunning, but rather it is that connections are also made alternately among the groups in the universe to allow for selectivity.

HELM: In terms of the total viability, it is not just one group and perhaps two outliers that count. It is the whole universe.

BICCHIERI: Ultimately yes, that is true. But the point is, that in one particular year they will look in one direction, and at that particular time there would be an extension of this connection. Whether that connection goes in another direction the next year is something else again.

ECONOMIC VIABILITY OF BANDS

LEACOCK: Relating now to economic viability, the Naskapi tent group of 15 to 20 people is not economically viable because starvation is a recurrent threat. The group has to depend on others from time to time.

GARDNER: Case material was presented earlier regarding relatively dispersed households: Rogers' example and that of the Shoshoni. This situation occurred even at the time of maximum aggregation. It seems to me that the Athapaskan and Algonkian people, especially, have generally had a social distance between people and no really close emotional ties. To what extent do groups split up because of social rather than economic reasons? To what extent is there a scattering of people not because they need to scatter to get food, but because they just do not want to be too near others?

Another case might be the Siriono, who are always suspicious of other people stealing food and who will not share food. They also keep their distance from others. It may be something in the social structure that accounts for this.

DUNNING: That is one aspect of it. Another surely is an economic one, although that too could be defined in social terms.

HELM: I believe that we would find differences in clumping or dispersal from region to region.

EGGAN: We should probably consider sharing practices in this context, as social sharing related to cycles of want.

In Asia there is variation in these practices from south to north. In the north, if you lose your reindeer herd, a small herd is made up by contributions from other families. Farther south a man becomes a slave when he loses a herd, and he herds someone else's reindeer. Does this happen in the New World from the Eskimo southward? In the Plains, a man who has lost his horses will be loaned or given horses but is expected to reciprocate in one way or another. What happens in the intermediate area?

HELM: Is there any case where, under conditions of want, one household may have an abundance of food and several households in the settlement who are less fortunate would not get shares?

GARDNER: The Siriono never share. They hog their food.

DAMAS: I have seen cases in the Eskimo area where one extended family had an abundance of food for their dogs whereas the next extended family in the same village had dogs that were starving.

SLOBODIN: There are two things to be noted here. First of all the Siriono are always hungry, and secondly, as has been noted in the case of the Eskimo, their meanness applies to dog food only. I wonder if we have cases among groups at this level of integration where people allow others to actually die out? Is there exploitation in the serf sense?

DAMAS: I can list the ways in which food is shared in the three Eskimo regions where I have worked. They all have fairly complex rules, which vary from one region to another. I have said something about this in my paper. I do feel that, in addition to these rules, and with the exception of dog food, there is a high value placed on generosity in this matter and that ultimately no one in a given encampment would be allowed to starve if there was food to share. At the same time, factions occur, and an Eskimo would go much sooner for help to the faction who were kin or at least congenial to him. There is also the matter of hospitality. You can eat at will in the house of your host if you happen to be travelling and staying with a family.

McCLELLAN: The Tutchone chief would see to it that each family in the summer grouping would get its share of meat. At the same time he would regulate exploitation by telling highly successful hunters to stop hunting so that the less successful would have a better opportunity to catch game. I wonder how often a headman could see to it that the exploitation is fair.

EGGAN: I think that it is clear from the discussion that sharing of food in various ways is an important cultural phenomenon which helps maintain the economic viability of the band — helps maintain a viable unit.

SIZE OF BAND TERRITORIES

SLOBODIN: The question that we should ask at this time is: What is the size of the territory within which these groups operate? We northern Canadian people are used to enormous territories; for instance, the first time I ever took a dog-team trip we covered a 500-mile round trip on some chore or other.

GARDNER: Five hundred miles in South Asia would take you past dozens of tribes. The situation is peculiar. Paliyans gather food at altitudes between 1,000 and 4,000 feet. Their territory is usually strung along a steep slope, a ribbon about half a mile to a mile wide except where there are valley indentations. Villages made up of two or three former bands are located along the main range every 8 to 10 miles. Where there are still bands, they are spaced about four miles apart along the range, or double this distance if the valley indentations are taken into account.

HELM: The situation that you describe is practically settled by northern standards. I mean, I am not sure they are really bands.

GARDNER: This concentration of about twenty people per square mile is an astonishing ratio for hunters and gatherers, a very abnormal situation. The yams are heavily exploited so that a certain amount of movement is necessary to prevent an area's food supply from being exhausted. Other movement can be referred to the social control device previously discussed.

BICCHIERI: The Hadzapi total about 500 and live in a scrub forest 50 miles wide and 200 to 300 miles long, or about 10,000 square miles in area. This gives an average of about 20 square miles per person. The groups range in size from 20 or 30 to 60 people.

WILLIAMS: The Birhor groups, averaging 25 in number, would be located about 7 to 10 miles apart. There are 600 people in an area of about 1,000 square miles. That would give an average of about 50 square miles to each group or 2 square miles per person.

SLOBODIN: Those of us who worked in northern Canada are conscious of the great historical concentration of population, especially in the last generation, so that these large territories indicated on the ethnographic maps are really the largest functioning bodies of people who, you might say, had a franchise on the territories. The trapping party that I described to you was an example of one returning to a traditional territory. However, I wonder sometimes whether this is entirely unprecedented in history in being some kind of intensive circumstance such as the intensive impact of European civilization. I wonder if some of the northern peoples have not been concentrated much in the past. Certainly this is true of the western Eskimo and among the northern Athapaskans. Those along the western Cordillera were fairly densely settled, for a long time. For instance, the Peel River Kutchin population was distributed at about one person per 34 square miles, and some of the Naskapi bands had one person for 50 square miles.

EGGAN: In the northern bands there is even up to 300 square miles per person.

SLOBODIN: I do not think that these figures necessarily represent populations that are traditional, and even those that can be precise do not represent the area that was exploited efficiently, except perhaps during the fur trade when the territory was more efficiently exploited.

SURVEY OF BAND SIZE

SLOBODIN: Perhaps at this point, keeping these various criteria for bands in mind, we might try to arrive at the size range of the units which we can call bands.

McKENNAN: I should like to give some figures on Athapaskan bands that are culled from the literature based on the earliest contacts. For the Ingalik there was an average of 77 people in several winter villages in 1834. Osgood gives 15 to 65 for the Tanana. My five Tanana bands number from 15 to about 45 with an average of 30 people. Murray in 1847 gives the total number of males for the Han as 57. My Chandalar bands vary in size from 15 to 65 people for the three bands at the time of my visit. I hesitate to speak for the Peel River Kutchin because Slobodin uses different groupings as bands, but as I recall they would number 70 to 80. For the Kaska, based on Honigmann's figures, the Deep River group had 175 people. This is the only figure that runs above 75 or so. For the Carrier, Harmon and Morice give 50 to 55. Perhaps all these observers were not using the same concept of a band, but I am impressed by a certain consistency of the figures with the exception of the Deep River Kaska, and it is likely that the figure of 175 for that group represents a modern trading post assemblage.

HELM: In the Mackenzie Drainage area for the Lac la Martre band of the present day, I have 21 recorded marital pairs or about 100 people. Back in 1911 there were 17 heads of households with an average of 5 persons per household or about 85 people for a regional group. These are what I call regional bands—they generally coalesced. For local bands the figures are much smaller.

McCLELLAN: My regional figures for the Southern Tutchone fall between 15 and 75.

DAMAS: The units that I have been calling bands, which correspond to Helm's regional bands, ranged from 46 to 159 and averaged 89 people, though as I indicated in my paper a more truly aboriginal figure would have been higher. Perhaps in the 19th century the range could have generally been 75 to 150 or 175 with a rough mean of 100 or slightly higher.

Regarding your local bands these would appear to correspond to the more recent hunting-trapping villages. These vary in size from an average of 35 (maximum 60) for the Iglulingmiut and an average of about 15 for the Copper Eskimo.

Did Steward not talk about composite bands of 250 for the northern Athapaskan area?

HELM: He was remarking about the extraordinary size of the Mackenzie bands. His figures came from Father Morice, who took them from Petitot and who, I am sure, was referring to the group which traded at a particular post and could have comprised two or three regional bands.

LEACOCK: The probable band size of the pre-fur trade Algonkians would fall into a 35 to 75 range. That would, perhaps, be 3- to 6-tent groups.

GARDNER: I have some figures here, including seasonal variations for some groups. The Semang of the Malayan Peninsula are dispersed in groups whose maximum size is from 20 to 30. The Andaman Islanders groups range from 30 to 50. The Caribou Eskimo groups average 13 in winter and 32 in summer. The northern Shoshoni, after the introduction of the horse, ranged up to 300. The Chenchu had 2 to 13 houses per settlement, and the Yahgan group consisted of 1 to 3 families. The Mungin were 25 to 50 and also 60 to 70, but Thompson found groups of over 100 at some seasons. For the Walbiri, Meggitt maintained that he found family-sized aggregations during part of the year and 100 to 330 people at other times. The Wintun groups are under 500. The Ona ranged from 40 to 120. The Siriono are dispersed in families during one season and number 60 to 80 in their largest aggregates. Hallowell's figures for the Saulteaux are 12 to 18 dispersed and 50 to over 200 in the largest aggregates.

DAMAS: There appears to be a spread in the figures from under 20 to several hundreds with perhaps a clustering between 50 to 200.

HELM: I think, however, that we are considering together both the local group and the people who might be regarded as comprising a band but who might not ever group together completely.

DAMAS: This is perhaps true, but still we have narrowed a range to manageable limits. Some of the figures that have been quoted are based on second-hand information and indeed are given without much uniformity in criteria. There is doubtlessly an intergrading in size between the two sorts of bands that Helm has mentioned. I think that our discussion of criteria for bands should lead us to a more critical examination of some of the figures that have been given for band size in various parts of the world. After this has been done, we can combine like entities and arrive at more meaningful comparisons of figures. After that is accomplished, we can begin to make generalizations about variation in size in relation to the type of economy, for instance, the hunting versus gathering ratio, and try to make some statements about a given carrying capacity influencing an optimum band size in a given region.

REMARKS ON THE METHODOLOGY OF BAND COMPOSITION ANALYSIS¹

By JUNE HELM

RÉSUMÉ

Plusieurs genres de problèmes ayant trait à l'organisation socio-territoriale des sociétés vivant en bandes sont présentés en fonction de la tentative d'analyser la composition des bandes chez les Athapasques du Nord. En tant que groupe humain, la bande est de nature à la fois territoriale et sociale. L'organisation de la bande est par conséquent (d'après Chang) à deux dimensions: d'une part, celle d'un *mode d'établissement*, qui concerne la forme que revêt l'occupation humaine par rapport à sa localisation et à sa juridiction; d'autre part, celle d'un *mode communautaire*, qui se centre sur les relations sociales des occupants du territoire plutôt que sur le territoire lui-même. Il est tenu pour acquis que les lignes de force qui agissent de façon immédiate sur les modes d'établissement dans les sociétés vivant en bandes dépendent du mode d'exploitation: les lignes de force sous-jacentes aux modes communautaires jaillissent directement de relations sociales symboliques qui servent d'indice de culture. Toutes les formes de combinaisons socio-territoriales mises en oeuvre par les membres des sociétés vivant en bandes doivent être embrassées avant que l'investigateur puisse se targuer d'avoir trouvé une définition opérationnelle acceptable de l'unité sociale appelée «bande», considérée comme distincte d'autres formes ou d'autres niveaux d'organisation socio-territoriale. D'après les constatations que l'on a pu faire chez les Athapasques, il semblerait que l'influence de la parenté, celle de la parenté au premier degré surtout, soit décisive dans l'établissement des démarcations socio-territoriales. Sans doute est-elle généralement décisive dans les sociétés vivant en bandes. L'auteur a trouvé que l'analyse des groupes socio-territoriaux, étudiés en fonction des liens de parenté du premier degré au niveau du couple conjugal, constituait une méthode valable pour détecter et ensuite pour évaluer quantitativement le type de composition communautaire chez les Athapasques du Nord. La question se pose de savoir si cette méthode pourra encore servir lorsqu'il s'agira de comparer les sociétés entre elles. Une tentative d'exploration dans cette direction est amorcée dans un article du même auteur qui fait suite à celui-ci.

My comments opening the general session on the Methodology of Band Composition Analysis are based on my efforts to understand the nature of the social organization of Northeastern Athapaskan (Déné) bands. I address myself to the broad questions that arose in attempting to deal with Athapaskan band composition with the hope that my particular efforts at resolution may prove relevant to the problem of socioterritorial organization in band societies in general.

The first problem is to identify the domain that we are considering. I take the immediate unit of concern to be the band, rather than the band society,² the difference being understood to be the difference between 'the

community' and 'the society.' What is a band as a component of a band society? What do we need to know about it? At the broadest, let us grant that a band as a human group is both territorial and social in its nature.

By distinguishing *settlement pattern* from *community pattern*, Chang (1962) has cut through the crust of confusion that has too often obscured the questions appropriate to the task of identifying and analysing bands. Settlement pattern for him refers to "any form of human occupation of any size over a particular locale for any length of time with a purpose of dwelling or ecological exploitation. The term conceptually stresses the locale rather than its inhabitants" (Ibid: 29). Chang here separates *territorial* aspects of the socioterritorial entity. Community pattern, Chang distinguishes as the *social* aspect of the socioterritorial entity: "the concept of community, a social group, centres around the occupants of a locale rather than a locale *per se*" (Ibid: 33).

Most broadly, we may predicate that in hunting-gathering band societies the directives underlying settlement patterns are based on the *exploitative pattern*, the exploitative pattern being the total set of activities in the acquisition of life's goods through the application of technology upon environment. We may further predicate that directives underlying community patterns stem most immediately from significant social relationships, as culturally defined.

Can one reasonably talk about *how* people ally themselves spatially, by whatever cultural directives, until one knows *what* the spatial, territorial arrangements of the population are? Settlement pattern, I have suggested, is the territorial aspect of this socioterritorial entity with which we are concerned. But taking Chang's emphasis on locale, and his recognition that members of the community at some moment may be scattered ("over a particular locale") and not in one single compacted settlement, the question then becomes which or how many settlement arrangements out of all that may occur through the year and the years are territorial manifestations of 'a band.' I think none of us, for the present at least, would say that a band exists only when all its members are together. What, then, of groupings that come together only seasonally and that are not necessarily composed of the same personnel from one year to the next? This brings the question of the kinds of territorial groupings—settlement patterns—that occur *in toto*.

Are there kinds of socioterritorial groups in band societies that we shall not call bands? Among the Athapaskans there may exist for as long as a month or two an all-male trapping or hunting party. Presumably we do not accept this as a band because our concept of the band unit refers to a more nearly complete social complement of persons involving women and children organized with the men into some sort of family units. But what of the clustering of two or three nuclear families, which, on the one hand, endures or regroups seasonally for a few or many years and, on the other hand, is somehow identified or allied with contiguous populations with which the group is not in co-settlement? Do we say this is merely one of the scattered camps of *the* band, or do we treat it as one kind or level of band organization in a society that is further comprised of other kinds and (or) levels? At the other extreme, in what way is the band, however specified, articulated with other like components of the total society?³

It is at this point that we fall back on *community* patterns or, more broadly, patterns of social relationships, collating them, so to speak, with settlement patterns in order to decide, if we can, what we shall define as 'the band' as a social-structural entity. For the Northern Athapaskans, at least, we first have to identify all social-*cum*-spatial arrangements, not excluding the most transitory and sex-and-age imbalanced spatial groupings. Only then may we attempt to decide what kind of socioterritorial structure shall be designated—for the immediate ethnographic purpose—the *band* among perhaps other levels or kinds of socioterritorial arrangements within the society, and to comprehend how the band and (or) kinds of 'bandings' are composed internally and are related to other like groupings. For the Mackenzie Drainage, Slobodin's (1962) works on the Peel River Kutchin is the only fully explicit report in this line that I know of.

Let me illustrate these questions with an example from the Dogribs, with whom I have most recently worked.⁴ The Marten Lake Dogribs have been identified in the literature since 1790, but the literature gives no evidence about their settlement arrangements or distribution over the area by which they are identified. The Marten Lake Dogribs are at present in one nucleated settlement centred around a government schoolhouse, but it is apparent that up to about seven years ago all the Marten Lake people were not, most of the time or perhaps any of the time, in a single nucleated settlement, sedentary or mobile. None the less they were known as the *tsontigotin* to themselves and to others—the "People of Filthy Lake"—a group comprised of families living in and exploiting the territory around Lac la Martre. 'Visiting' families (and individual males) from other areas might enter the area, linking up spatially and socially (to be discussed below) with established residents. Continued residence in the Lac la Martre population would lead in time to the identification of such persons as 'Marten Lake People.'

The Marten Lake People, then, were a population—not necessarily all together at any time of the year but whose personnel were in contiguous relation to one another—exploiting a zone with ill-defined boundaries. If we are willing to grant that these people, by their own and others' identification, were a kind of territorial group, were they a band? I have so called them—qualifying them as a 'regional band'—first and foremost on the basis of social identity rather than on the basis of co-settlement or sharply drawn band-boundaries on the physiographic environment. But if I am to understand how the community patterning functions *in toto*, I still have to know not only the grossest social identity—the Marten Lake Dogrib—but also all the varieties of settlement patterns of all segments and sections of these people through the seasons and through the years. This brings me to identify segments of the total population—sets of nuclear families enduring in co-settlement for at least a few years—which I call local bands. I am also made aware of other segments composed of perhaps only one or two families that lived apart from others by season or for a few years and for whom I find it difficult to assign any term. I further distinguish the foregoing kinds of groups from those seasonal groupings at the fishery, or on the trap-line, or on the hunt that I have called task groups (Helm 1965; 1968). (Permit me to exclude the latter, on the basis of their more transient social identity, from further consideration at this time.) In the more enduring groups sharing

some form of socioterritorial identity, I have looked for some kind of patterning in the kinds of statuses, significant social relationships. I seek community pattern or patterns.

What we seek first in band community patterns are social directives for residence alignment. Ethnographic experience leads us to look first for those social directives based on kin relationships. As the topic of kin relationships is sufficiently complex in itself, I will limit the discussion to this focus.

The significant social relationships of any Ego in a band society are not necessarily coterminous within or bounded by the single settlement. Northern Athapaskans, for example, have kinsmen in other areas and with other territorial groups than those in which they reside at the moment. Herein lies the potential for alternatives or choices in band-residence.

Fischer and Goodenough have given us very cogent leads in this matter of analysing kin-based residence, leads as applicable to inquiry into community residence as into household residence. Fischer's (1958) concept of residence sponsorship of the *individual* gives us an immediate technique for socially relating each individual to his residence location at any particular moment in time. At the time of entry into residence in a household and(or) community, what is the kin status of each individual to his(her) immediate sponsor—that of self, wife, husband, son, daughter, brother, sister, mother, father. Although in most societies the conjugal pair ordinarily establishes itself as a single unit in residence, Fischer enjoins us to recognize that it is ordinarily one spouse in the pair that serves as sponsor of the other into any pre-established household. For marriages between members of different communities (bands or hamlets, etc.) the same consideration holds. We can, for our present concerns, take spouse sponsorship for granted; then we can turn to the question of what type of kinsman is serving as sponsor of the sponsoring spouse.

This leads us to Goodenough's (1956) point that the ethnographer errs when he attempts to analyse and categorize residence behaviour in a particular society in terms of the set of residence 'rules' developed in comparative studies—This-or-That-Kind-of-Locality. Rather, for each society under study the ethnographer must "try to conceive categories of residence and criteria of choice which give the simplest and most accurate account" of the behaviour of the members of that particular group. Furthermore, as Goodenough (1962) has made us aware from his study of Lakalai, decisions affecting residence may come into play that stem, not solely from kin statuses, but from self-interests in security, wealth, useful alliance, and so forth, and that are attained by selecting residence sponsors among allowed alternative kinsmen. That is, within 'rules' of residence alliance predicated on kinship, a number of alternative decisions as to actual alliance may be available.

Let me return again to the Mackenzie Athapaskans to illustrate both kinship directives and the play of alternatives in post-nuptial residence decisions of conjugal pairs. Analysis of the kinship composition of four bands, local and regional, shows that there is no married adult in such a group that does not have a primary affinal (marital) tie or primary consanguineal tie to at least one other member of the group (Helm 1965). But when I say some kind of primary consanguineal and (or) affinal tie, a range of choice is at once implied. From the position of married male Ego, a man may marry within

the local and (or) regional band where he resides with his parents. Or he may bring his alien wife back to his band. Or he may elect (after bride-service) to stay with his alien wife's band. Or he may elect to join the band where his brother or sister, married to a member of that band, resides, and so on.

From the statistical and structural evidence in the Déné bands known to me, I can say that a Northern Athapaskan does not settle in a band-community unless he (she) has, or establishes through marriage, a primary kin tie to some already-resident member of that band. I can say which tie or ties exist within the band for each person: that of husband to wife, wife to husband, brother to brother, brother to sister, sister to sister, son to parents, daughter to parents. When one spouse of the conjugal pair has 'married in' as an outsider, I know that the primary ties of the resident spouse were 'sufficient', at least, to effect the residence choice of the pair. But I cannot for each particular residence choice always say which tie or ties out of a multiple set have been critical, or why. At this point, one must go, as Goodenough counsels, beyond the census data and beyond the genealogical data to get at the actual residence decisions made within the kinship milieu—from among those primary kin types that may equally serve as sponsors. Regarding these, I have only occasional rather than systematic knowledge. For Athapaskan men, I know that a dominant father or brother, the personal amiability of a brother-in-law, or the lack of primary relatives of one's own may be the immediate and sufficient factor in residence decision. Recruitment through kin lines around particular leader personalities also seems to play a role.

One of the problems in residence analysis for Northeastern Athapaskan bands, and I suspect that this holds for many other kinds of societies, is not only that Northern Athapaskan socioterritorial groups were, and are to some extent today, spatially mobile groups, but that nuclear families are spatially and socially mobile between groups. The only way I have found to resolve the problem of fluctuating and altering socioterritorial membership, especially when the factors in the residence decisions of all personnel are not known, is to analyse community composition at any given slice of time. A socio-territorial unit among the Athapaskans may not contain the same personnel now as it did one or ten years ago. However, as I indicated earlier, analyses of kin relationships within the 'same' local or regional Athapaskan band-community at different periods of time produce the same pattern, that of a chain of primary relative links, consanguine and affinal, by which each person is tied once or several times into the community whole. Furthermore, the equal number of male versus female 'ends' of each link yields evidence of bilateral forces in post-nuptial residence alignment in these Déné groups. I have found analysis in terms of primary relative bonds between conjugal pairs to be a viable method for perceiving and quantifying the pattern of Northern Déné community composition. Now the question is, will it serve cross-societal purposes and for cross-societal comparisons?

NOTES

1. Notes are later additions to the text of the oral presentation.
2. Most broadly, in this conference we are considering band societies, which are not coterminous with bands *per se*. By anthropological convention, band societies are

set apart from non-band societies of all kinds. But I do not believe the features conventionally used to identify a band society — nomadism, lack of central tribal authority, and hunting-gathering pursuits (although here sometimes pastoralists, Pearson's Kōnkāma Lapps, for example, may be included under the rubric of bands) — make the problems of methodology in the analysis and classification of community composition inherently different from those encountered by perceptive ethnologists working in non-band societies.

3. This conference has brought into the open the whole problem of specification of the 'band' unit in any particular band society, notably, that the 'band' as a unit identified by Ethnographer A for Society X is not necessarily the same order of phenomenon, territorial or social, as that identified by Ethnologist B for Society Y. To choose two pronouncedly contrasting cases, the small, cohesive group that Lorna Marshall (1960) identifies as the !Kung 'band' cannot be taken to be immediately comparable to Slobodin's (1962) 'band' of the Peel River Kutchin. The latter 'band' is not a single spatially cohesive unit; it is in fact coterminous with the society of the Peel River Kutchin. If congruent kinds of groups can be identified between the !Kung and Peel River Kutchin, we cannot assume *a priori* that the congruence is between the two units that have been termed 'band' by the respective ethnographers.
4. The Dogrib *in toto* may be called a 'tribe' (being composed of several regional bands). I have elsewhere (Helm 1965: 384) suggested that without political integration or aggressively defined and defended boundaries, the largest socioterritorial unit of the band society, the 'tribe' or what-have-you, is to be defined ideologically only by social identity and structurally by the "greatest extension of population throughout which there is a high concentration of marriages between members." At this time, I am not prepared to develop this point more rigorously.

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A METHOD OF STATISTICAL ANALYSIS OF PRIMARY RELATIVE BONDS IN COMMUNITY COMPOSITION

By JUNE HELM

RÉSUMÉ

Partant du postulat que les liens de parenté du premier degré sont significatifs lorsqu'il s'agit de déterminer la composition communautaire des sociétés vivant en bandes, on présente une méthode statistique destinée à établir l'espèce et le nombre de liens de parenté consanguine du premier degré qui s'établissent entre les couples conjugaux vivant en corésidence dans la communauté. Les observations à l'appui de la démonstration ont été faites dans un ensemble de bandes d'Athapasques et dans un autre ensemble de bandes d'Iglulingmiut. On présente ensuite au lecteur une deuxième série de méthodes quantitatives, dans un effort d'exploration destiné à élaborer une méthode qui permette de comparer, à travers les sociétés, l'importance de l'homme par rapport aux statuts de la femme impliqués dans les liens de parenté au premier degré entre les couples conjugaux vivant en corésidence. Les résultats statistiques obtenus pour l'ensemble des bandes d'Athapasques sont comparés à ceux qui ont été obtenus pour les Iglulingmiut. Ces résultats confirment les constatations ethnologiques obtenues par des procédés différents, à savoir que, chez les Iglulingmiut, c'est la résidence masculine qui l'emporte, alors que, chez les Athapasques, la résidence du couple est bilocale. On suggère que le profil statistique total de la composition communautaire (en fonction des liens de parenté du premier degré entre couples conjugaux corésidents) pourrait être utilement employé lorsqu'il s'agit d'établir une stricte comparaison entre les unités sociales. Il est néanmoins rappelé que, dans la vie de tous les jours, un grand nombre de facteurs peuvent intervenir, qui empêchent les modèles statistiques de reproduire ou même de refléter les normes unilocales ou bilocales ou encore d'autres lignes de force qui conditionnent la résidence. En d'autres termes, le but de la méthode statistique, dans la description de la composition communautaire, est d'indiquer de qui et de quoi se compose une communauté, en fonction des liens de parenté du premier degré, et non pourquoi elle est ainsi composée. Le pourquoi se trouve dans des motivations et des valeurs culturelles qui doivent être recherchées à l'aide d'un autre genre d'enquête et d'analyse ethnographique.

At this point in the session on Methodology of Band Composition Analysis, the participants' attention was directed to tables and diagrams of the kin composition of a set of Northern Athapaskan (Déné) bands which were illustrations of techniques for quantifying primary kin bonds within band-communities; a discussion followed.

The discussion centred on two major themes: (1) the significance of primary kin bonds between community co-resident conjugal pairs, in Helm's Athapaskan sample and in the band societies studied by the other participants;

and (2) problems and possibilities in a method of scoring male and female statuses in the primary consanguine bonds among co-resident conjugal pairs.¹

The set of quantifying procedures demonstrating the first theme has since been published (Helm 1965). Part I is drawn from that material.

The second set of quantifying procedures, presented in Part II, was offered to the conference as an exploratory effort. They are a further manipulation of some of the first set of analyses and represent an attempt to develop a procedure which permits cross-societal comparisons of the weight of male as compared to female statuses involved in primary bonds between co-resident conjugal pairs. At the conference the technique was presented in two steps: first, a count of the number of times a male status appears in a primary relative bond linking one pair to another pair in the community, as compared to a female status; secondly, a conversion of the raw scores to percentages. A third level of conversion, developed since the Conference, is the male index — the ratio of male to male-plus-female statuses. It is incorporated in this paper.

The basic procedures to be presented should serve equally for the analysis of any kin-based residence group that is commonly comprised of more than one conjugal pair, e.g., extended family household. For present purposes, however, the exposition will be phrased solely in terms of the band-community as a residence group.

The immediate impetus that led to the attempt to develop a technique of scoring male versus female sex statuses in primary bonds from one conjugal pair to another stemmed from a number of earlier discussions between Helm and Damas regarding the problem of assessing, from genealogical evidence, the amount of practice of virilocal residence in community units, as it might afford comparison with the presence or absence of a stated norm or 'rule' of virilocal residence (or any other residence rule) in a society. From the Iglulingmiut, Damas had obtained statements that virilocal residence—that is, residential solidarity of a male with his brothers and father—was the preferred residence pattern, and he felt that the inspection of the genealogies of co-resident Iglulingmiut did indeed indicate that the majority of the Iglulingmiut conjugal pairs conformed in practice to the stated norm (*see* Damas 1963, chapter 4, for his methods of analysis). Helm (1965) had developed the original set of quantifying techniques on Athapaskan data to demonstrate that the 'bush' community groupings of the Mackenzie Athapaskans are made up of conjugal pairs knit together by bilateral primary consanguine ties. The question posed, then, was whether it was possible to demonstrate by quantified measures a *de facto* virilocal weighting in Iglulingmiut co-residence and a disparity in this respect between Damas's Iglulingmiut groups (putative virilocal emphasis) and Helm's Athapaskan groups (putative bilocal emphasis). To achieve this cross-societal comparability, the method of conversion to percentage of male versus female statuses and, later, the male index were evolved.

The particular data provided by Damas before the Conference were on large bands from three Eskimo societies. They were not so directly comparable to the Athapaskan sample, in terms of community sizes, as is a set of data on the kin composition of the Iglulingmiut bands published by Damas (1963). In this presentation, therefore, the composition of the Iglulingmiut bands at one horizon, 1921–22, has been selected as demonstration material.

For one Iglulingmiut settlement, Iglulik, the 1949 composition is appended to Tables 2 and 4, to afford comparison with Iglulik 1921-22. Both Iglulingmiut and Déné data represent compacted band-settlements at restricted intervals in time.

Part II

PRIMARY CONSANGUINE BONDS BETWEEN CONJUGAL PAIRS IN COMMUNITY CO-RESIDENCE

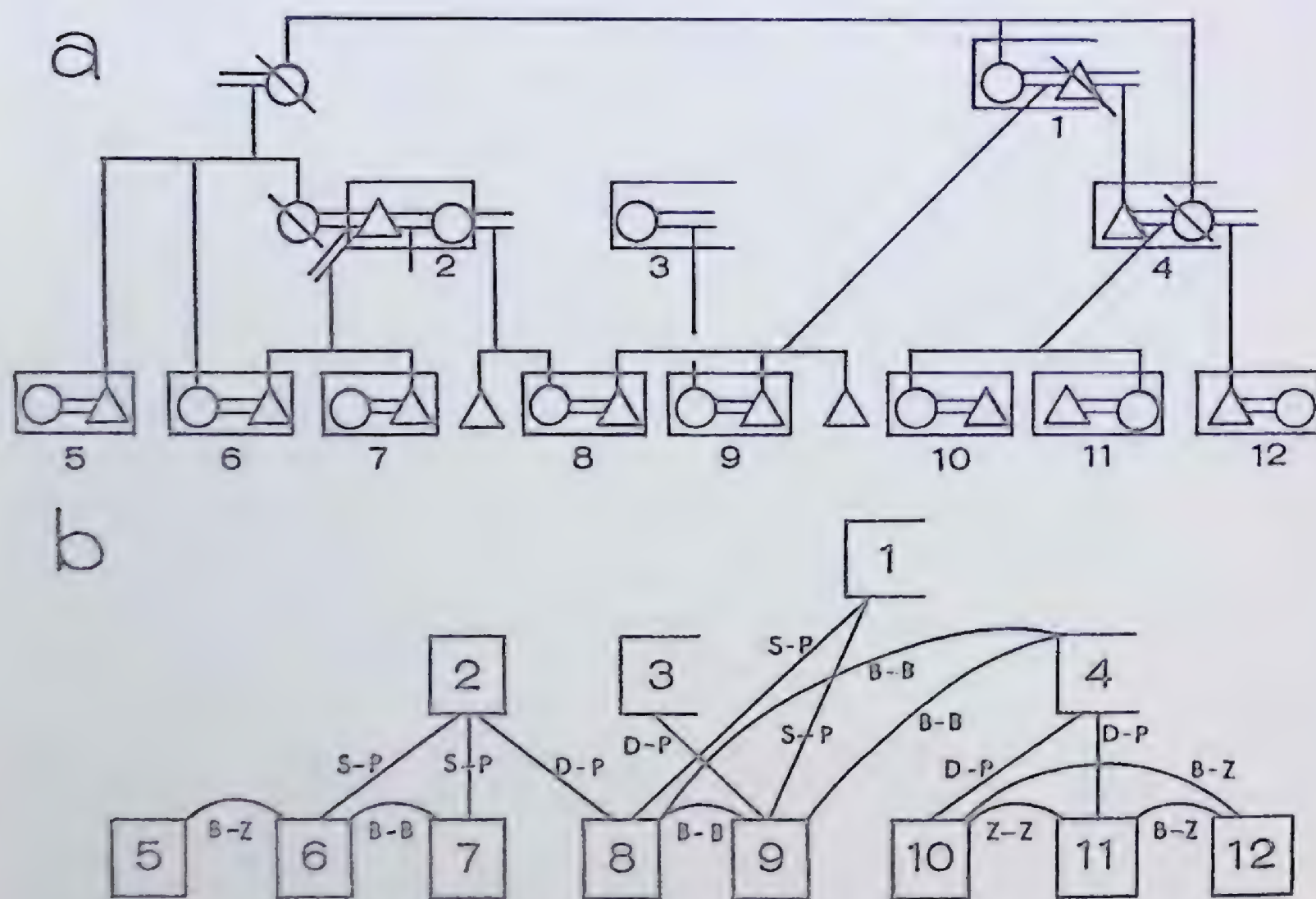
Method of reckoning bonds

Figure 1 illustrates the method of analysis of community composition of the several Déné bands referred to in the paper "Methodology of Band Composition Analysis," *supra*. It presents two kinds of diagrams of the kin composition of one Déné band-community.

FIGURE 1—*Lynx* Point 1959 (*Déné* Band C2)

a diagram: closest genealogical relations between living married (and widowed) persons.

b diagram: dyadic relations of the primary consanguine order between conjugal pairs as units.



open-end marriage sign = deceased spouse never resident

The *a* diagram of Figure 1 is a standard genealogical presentation featuring living married adults, by sex. It excludes unmarried juveniles (whose automatic primary tie to and co-residence with their parents need not be illustrated) but includes a minimally sufficient number of formerly resident deceased siblings or deceased lineal ancestors to show the most immediate genealogical connectedness between living members in the community. Enclosed in 'boxes' are the living conjugal pairs and the surviving spouses of former conjugal pairs that are resident-members of the community at the specified time period.

The *b* diagram in Figure 1 was evolved in the search for a more parsimonious key to community composition. In the *b* diagram, each living conjugal pair (or survivor of a former pair) is treated as a compositional unit, indicated by a square. (The few grown persons not yet married are irrelevant and are excluded.) Every primary consanguine relationship extending, by virtue of either spouse, from each pair to any other conjugal pair(s) in the community is indicated by a curved line for a sibling bond (B-B, B-Z, or Z-Z) or a straight line for a child-parent link (D-P or S-P) between the two units. In other words, the type *b* diagram expresses *dyadic relations of the primary consanguine order between married couples as units*, rather than between individuals, and reveals that every living conjugal pair in this community is linked in a primary consanguine relationship by at least one of its members to at least one other living person in another conjugal unit. This statement holds for almost every living pair in all of the Déné communities analysed (*See* range, mode, and mean columns of Table 1, and further explication in Helm 1965: 370). Treating these persons tied together by the primary affinal bond, i.e., the conjugal pair, as a residence unit eliminates from the diagrams the symbol complexity of primary affinal (conjugal) ties. We then can proceed to view the structural mode of band composition as one of primary consanguine linkage between conjugal pairs as the units of membership.

Tables 1 and 2 present the scoring of features of primary kin connections revealed in the *b* diagrams for a set of Athapaskan bands and for a set of Iglulingmiut Eskimo bands at specified intervals in time. (Compare the counts recorded in Table 1 for Lynx Point 1961 [Band C2] with the various sibling-to-sibling and child-to-parent notations in the *b* diagram for Lynx Point 1951 in Figure 1.) For comparability with the Iglulingmiut units, only those Athapaskan groups in compacted co-settlement at the stated periods are presented in the present Table 1 (cf. Table 1 in Helm 1965: 369).

TABLE 1
Mackenzie Déné: primary consanguine bonds between living conjugal pairs

Socioterritorial units (by specified temporal horizon)	N _p	---A---										N _a	Percentage of primary consanguine bonds out of total dyadic relations between pairs
		Number of primary bonds					From each pair to others	Mean	Mode	Range	Number		
		Between pairs											
		Sibling-to-sibling			Child-to-parents								
		B-B	B-Z	Z-Z	S-Ps	D-Ps	T						
A Little Chicago 1957: (Hare)	4	6	0-2	1	1.5	0	0	1	0	2	3	6	50.0
B Colville Lake Segment 1957: (Hare)	8	18	1-4	not*	2.3	6	0	0	0	3	9	28	32.1
C1 Lynx Point 1911: (Slave)	4	14	3-4	3,4	3.5	0	0	3	1	3	7	6	100.0
C2 Lynx Point 1951: (Slave)	12	32	1-4	3	2.7	4	3	1	4	4	16	66	24.2
D2.1 Marten Lake North Segment 1925-52	11	38	1-6	3,4	3.5	2	7	3	3	4	19	55	32.7
D2.2 Marten Lake South Segment 1925-52	12	24	0-4	2	2.0	3	5	2	0	2	12	66	18.2
D3 Marten Lake 1959: (Dogrib)	21	80	0-7	4	3.8	9	10	5	6	10	40	210	19.0
Overall mean					2.8								

*nc1 = no central tendency.

TABLE 2

Iglulingmiut 1921-22: primary consanguine bonds between living conjugal pairs
(from Damas 1963: 59-66)

Socioterritorial units (by specified temporal horizon)	N _p	—A— Number of primary bonds							N _d	—B— Percentage of primary consanguine bonds out of total dyadic relations between pairs			
		From each pair to others				Between pairs							
		Number	Range	Mode	Mean	Sibling-to-sibling		Child-to-parents					
						B-B	B-Z				Z-Z	S	D
Amitsug 1921-2	3	6	2	2	2.0	0	1	0	1	1	3	100.0	
Manirtug 1921-2	5	10	1-3	1,2	1.8	1	1	0	1	2	5	50.0	
Pingirqalik 1921-2	6	20	0-4	4	3.3	6	0	0	4	0	10	66.7	
Itibjiriac 1921-2	9	22	1-4	3	2.4	1	4	0	4	2	11	30.6	
Iglulik 1921-2	25	76	0-9	5	3.0	13	9	2	11	3	38	12.7	
Overall mean					2.5								
Iglulik 1949	16	44	0-6	2	2.8	11	2	1	6	2	22	18.3	

Scoring: explanation of Tables 1 and 2

N_p is the number of conjugal pair units. N_d is the number of dyadic relations between total conjugal pair units, i.e., $\frac{N_p \times (N_p - 1)}{2}$.

The reckoning is not between individuals or kin-types *per se* but between kinds of primary relative categories that may reasonably be suspected to influence post-nuptial community residence of the conjugal pair. To this end, the conception embodied in the type *b* diagram (see Figure 1), that of dyadic primary ties between conjugal units, is employed in reckoning links. Each conjugal pair is compared with every other in the community. Each time a primary relative bond extends from one pair to another, a count is recorded. In regard to sibling bonds, the sex of the linking members from each of the two conjugal pairs involved is noted, as the three main kinds of sibling relationships may be relevant — that is, whether the bond extending between two conjugal units is between two brothers, between a brother and sister, or between two sisters. Half-siblings are counted as siblings; so are step-siblings so long as the linking primary relatives in the parental generation are living.

In a lineal tie, that is, between an offspring in one conjugal unit and his (her) parents (forming another conjugal unit), the bond is counted only once. It is the linkage of each individual married offspring to his (her) *parents* as an indivisible unit (since father and mother are always together as a residence unit, no choice between them can be made) that is deemed to be a prime social nexus and potentially significant in influencing band recruitment. Accordingly, 'parents'—a single entity—are counted as a single category (P) and as sexually neuter (the sex of father and mother cancelling each other out). Sex of child, whether son or daughter, may be significant and is therefore noted.

In all calculations involving conjugal pairs, a pair is counted as 'living' if one of its members survives at the specified horizon. However, in reckoning consanguine ties between any two conjugal pairs, only ties linking two surviving consanguines in the two pairs are counted. Thus, a widow is counted as a 'conjugal pair' and scored with a 'P,' parent, (and in the sex coding system presented in Part II is coded as 'Neuter' in relation to her married children), but only her consanguine ties to other living persons are reckoned, not those of her deceased spouse.

Part II

MALE VERSUS FEMALE STATUSES IN THE PRIMARY CONSANGUINE BONDS BETWEEN CO-RESIDENT CONJUGAL PAIRS

Rationale of scoring by "sex-significance"

In any society, in so far as primary relative ties are significant in determining the residence alignment of married couples, we should expect to find the conjugal pairs in any community linked by primary ties to other conjugal pairs. This is the premise underlying the demonstration from Déné and Iglulingmiut data (Tables 1 and 2) presented in Part I. Further, if cultural rules or preferences stress patri-virilocal residence, we should expect to find

that, except within fully endogamous communities, (married) males will preponderate over females in sibling relationships and to their respective parental pairs. (Other kinds of norms or factors influencing alliance may intervene, however, as will be discussed *infra*.) Given a matri-uxorilocal norm, females should preponderate in like relationships.

In a hypothetical society requiring absolute adherence to a patri-virilocal residence rule in combination with community exogamy, married couples in community co-residence would be linked in primary kin relationships to other married couples of their own generation only through males (except in cases of marriages to sisters), and only the males would hold primary consanguine relationships to parent pairs. So also, with appropriate sex reversal, in a society holding strictly to a matri-uxorilocal rule of post-nuptial residence. In a society holding to a bilocal norm, males and females should figure (roughly) equally in primary links between conjugal pairs in community co-residence.

In the following paragraphs, I offer a method "to determine average values—thresholds" of sex significance in post-nuptial residence alignment—that is, a method of creating a statistical model (Lévi-Strauss 1951: 528). For a given society, such a statistical model of community composition should provide one means of assessing the extent to which behaviour conforms to conscious cultural norms—namely, unilocal or bilocal residence rules or preferences. In the absence of consciously held and explicated norms, the statistical models may none the less reveal patterning in residence choices.

Method of quantifying sex-significance

The operations presented in Tables 4 and 5 are a series of further abstractions from the counts of the three kinds of sibling-to-sibling relationships and the two kinds of child-to-parent relationships presented for the Déné groups and for the Iglulingmiut in Tables 1 and 2 of Part I.

First step: The first step is the conversion of the raw 'bond' scores between conjugal pairs to a count of male against female against neuter statuses obtaining at the 'end' of each bond linking two conjugal pairs. This yields a new set of raw scores, double in number to the count of primary bonds between pairs. Thus, one brother-to-brother bond converts in terms of the number of sex statuses involved in that bond into two 'male' scores; that is, as there is a brother at each 'end' of the brother-to-brother bond that links two conjugal pairs, two male sex statuses are involved in that one primary consanguine link and are so scored. A brother-to-sister bond, of course, yields one male score and one female score, and a sister-to-sister bond yields two female scores. This method of reckoning requires, however, that the parental pair be treated as a neuter category, as male-father and female-mother in the parental pair cancel one another in terms of possible sex significance in the post-nuptial residential decision of the child-plus-spouse. This results from adhering to the position that one cannot decide from statistical and genealogical evidence alone which member of the parental pair—that is, father or mother rather than both—may be the sex figure in that pair affecting the residential alignment of the married minus-one generation in that family line. Therefore, when a child-parent bond obtains between (some of the) members of two conjugal pairs, the child is scored by sex, male or female, since that sex status

may be the criterion for sponsorship of self-and-thus-spouse by self's parents (or sponsorship of parents by self), but the parents are sexually coalesced into a single neuter score.

In Table 3 the derivation of the scoring by sex (male, female, neuter) from the primary bond scores is shown for Lynx Point 1951.

TABLE 3
Example of conversion of primary bond scores to sex-status scores:
Lynx Point 1951 (Déné Band C2).

Primary bonds		B-B	B-Z	Z-Z	S-P	D-P	Totals	Percent-ages
		N						
Sex statuses								
Male	N	8	3	—	4	—	15	46.9
Female	N	—	3	2	—	4	9	28.1
Neuter	N	—	—	—	4	4	8	25.0

$$\text{Male index} = \frac{\text{Male}}{\text{Male} + \text{Female}} = \frac{15}{24} = .625$$

Second Step: Once the number and kind of sex statuses — male, female, and neuter — involved in each bond have been recorded as raw scores, they are converted into percentages of male compared to female compared to neuter (i.e., parental) sex statuses in primary relative ties in each community.

Third Step: To eliminate the problem of the neuter (parental) category, which says neither yea nor nay on the matter of sex significance in residential alignment, the male index was developed. The male index is the ratio of male statuses to combined male-plus-female statuses. For example, absolute adherence to patri-virilocal principles in an exogamic co-residence group would yield (except in the case of brothers marrying sisters) a male index of 1.0. When primary relative connections are honoured but no unilocal principle is held to, that is, when truly bilocal choices are operative, one community through slices of time or sets of communities at any moment in time should produce a set of fluctuations around the .5 level of the male index: the larger the number of cases, the closer the overall mean may be expected to approximate .5. There may, of course, be a number of other biases operating in a society without unilocal preference to produce a skew of the male index away from the .5 level in either direction, just as there may be biases that intervene in a putatively unilocal society to skew the male index from the ideal. Some of these are cited in the following section.

Turning to the materials of Helm and Damas that brought about these efforts (Tables 4 and 5), we see that the Iglulingmiut 1921–22 do indeed show a substantial preponderance of male statuses, expressed in the Iglulingmiut overall mean male index of .688 as against .431 for the Athapaskan sample.² In other words, the primary kin connections between conjugal pairs in the Iglulingmiut bands involve proportionately more than half as many again male statuses than do those connections in the Athapaskan bands.

TABLE 4

Mackenzie Déné: sex-statuses in primary consanguine bonds between living conjugal pairs

Socioterritorial units			M	F	N	Male index
A	Little Chicago 1957: (Hare)	N	0	4	2	.000
		%	0	66.7	33.3	
B	Colville Lake Segment 1957: (Hare)	N	12	3	3	.800
		%	66.7	16.7	16.7	
C1	Lynx Point 1911: (Slave)	N	1	9	4	.111
		%	7.1	64.3	28.6	
C2	Lynx Point 1951: (Slave)	N	15	9	4	.625
		%	46.9	28.1	25.0	
D2.1	Marten Lake North Segment 1925-52	N	14	17	7	.452
		%	36.8	44.7	18.4	
D2.2	Marten Lake South Segment 1925-52	N	11	11	2	.500
		%	45.8	45.8	8.3	
D3	Marten Lake 1959: (Dogrib)	N	34	30	16	.531
		%	42.5	37.5	20.0	
Mean male index						.431

TABLE 5

Iglulingmiut 1921-22: sex-statuses in primary consanguine bonds between living conjugal pairs

Socioterritorial units		M	F	N	Male index
Amitsug	N %	2 33.3	2 33.3	2 33.3	.500
Manirtaq	N %	4 40.0	3 30.0	3 30.0	.571
Pingirqalik	N %	16 80.0	0 0	4 20.0	1.000
Itibjiriat	N %	10 45.5	6 27.3	6 27.3	.625
Iglulik	N %	46 60.5	16 21.1	14 18.4	.742
Mean male index					.688
Iglulik 1949	N %	30 68.2	6 13.6	8 18.2	.833

Limitations and caveats

All that we assuredly have here is a quantified description of the facts of primary relative connections within communities. Tests of statistical significance are inappropriate for the data with which we deal.³ Furthermore, the indeterminacy of parameters precludes an *a priori* definition of how 'high'

the male index must be to be a 'significant' reflection of virilocal decisions or rules, how 'low' vis-à-vis uxori-local rules, or what the middle range must be vis-à-vis bilocal rules. With adequate cross-societal sampling there might eventually be established an empirical level which could serve as a datum point in comparisons.

Whether it will prove to be worth having this kind of description is at this time an open question. It seems to me that, for the purpose of the immediate comparison, the male-female-neuter percentages and the male index scores do permit a somewhat different and more precise perspective on sex-significance in the residence alignments of Iglulingmiut against Déné than that gained from impressionistic assessments of community genealogical charts.

The total statistical profile of community composition in no way replaces conventional data on and interpretations of genealogies (including decedents that may have originally sponsored one or more living residents), stated residence rules or preferences, histories of decisions and sequences of movements of conjugal pairs from one group to another through time (the best information to have, but difficult to obtain in sufficient quantity and quality), or any other kinds of data that might indicate possible factors and influences in residence decisions.

A number of factors may operate in 'real-life' to prevent statistical models from precisely replicating or reflecting unilocal or bilocal rules or norms. The several interventions noted below are evident factors in shaping community composition in several societies treated at this Conference: the list does not pretend to be exhaustive.

- (1) to the extent that community endogamy is practised, a unilocal norm for out-marriages will not be reflected, and the concept of either-or locality becomes irrelevant. For example, it appears that in a large Déné community (e.g., Marten Lake Dogribs) half of the marriages may be endogamous. In very small communities, the lack of possible marriage partners usually forces statistical exogamy upon the group, even in the absence of community exogamic rules.
- (2) A second concomitant of small size of socioterritorial units is the likelihood of sex imbalance in births. Under either virilocal or bilocal norms, a community of a few (say, three or four) families that produce mostly female offspring is eventually faced with an insufficiency of able males. If it is to endure as a socioterritorial unit, there must be recruitment of males, in practice often sons-in-law, as co-residents, as Dunning (1959) demonstrates for the viri-oriented northern Ojibwa of Pekangikum in the past. Such uxori-local recruitments will veer the statistical model of community composition from correspondence to the 'ideal.'

The 'weight' of small communities (and their special problems) in a total sample could be reduced by computing a single total male index directly from the total male and female statuses in the sample, rather than allowing the small community equal weight with the large by taking the mean of the male indexes of each community in the sample (as has been done here). By the former method, even closer statistical approxi-

mations to putative residence norms are obtained for the Iglulingmiut 1921–22 and the Déné, .743 and .512 respectively.

- (3) With no information other than that of the genealogical connections within community aggregates at a slice out of time, transients or visitors—for example, conjugal pairs that might be restrained by residence norms from ‘settling’—cannot be distinguished from residents as socially identified by the group.
- (4) As a variant of condition (3), the custom of bride service may produce a lowered male index, even under jural patrilocality. If the period of bride service is customarily for a year or two, the effect on the male index will be slight, as there will be but few instances of bride service in operation at any selected short segment of time. (One- or two-year bride service is still partially observed among the Déné, but in the analysed Déné socioterritorial units no case was in operation. The Iglulingmiut also practise short bride service, but statistically they none the less reflect their normative preference for virilocality.) However, when bride service is a matter of many years, the chances of a number of cases being in operation in any sample of communities in any segment of time are proportionately increased.⁴

To the extent that (5) importation of alien spouses’ parents or (6) marriage of brothers to alien sisters is practised, ratios of male to female to neuter statuses accordingly will be affected.

The purpose of the statistical profile is to tell us the ‘who’ and ‘what’ of community composition, not the ‘whys’ (although ‘who’ and ‘what’ may sometimes suggest inferences as to ‘why’). Therefore, even when such factors or conditions as cited above are known from the fuller ethnographic data, departure from the regular scoring procedures in order to accommodate such knowledge ordinarily does not seem warranted. Certainly it is not warranted when such knowledge is ‘hit-or-miss’ or otherwise incomplete.

As these quantifying techniques are applied to living conjugal pairs (or survivors of pairs) in co-residence at a slice out of time, their deaths, immigrations, emigrations, and re-marriages, as well as new first marriages, will serve to change ratios from one ‘slice’ to the next. Therefore, in order to determine average values there is needed a sufficient number of cases to provide an adequate sample of the socioterritorial groups in the total society at any one moment in time, and, ideally, also a set of slices through time for a set of communities. Damas’s sample is total for the Iglulingmiut at the 1921–22 interval (and for two other intervals, in 1949 and 1961–62; 1963: 66–99). The cases from the Déné were gathered opportunistically, and they do not meet the criteria for a proper sample, being neither total, genuinely random, nor stratified with aforethought.

As an addendum, it should be pointed out that the practice of polygamy presents no problem in analysis. One has merely, then, marital triads or quadrads, etc. along with marital pairs. Number and kinds of primary consanguine relationships from every member of the triad or quadrad to any and every other member of the co-residential unit are drawn in the same fashion as is done for monogamous marriages.

NOTES

1. Many of the questions raised and points made by the participants in response to the extemporaneous presentation of the method have been incorporated in this later, written version. Therefore, they are not repeated in the discussion section that follows. I can only make a grateful and blanket acknowledgment of the many contributions of my colleagues to the present paper.
2. Male preponderance also holds for the sets of samples from three Eskimo societies provided by Damas at the Conference. All are large communities (14 to 50 conjugal pairs). The three socioterritorial units of the Copper Eskimo yield a combined mean male index of .637. The 1960 Iglulik settlement of the Iglulingmiut has a male index of .759. The five units of the Netsilik Eskimo give the lowest mean male index, .598, two of the groups (male index .444 and .448) weighting on the female side to pull down the combined average.
3. I am indebted to Roland K. Hawkes for giving generously of his knowledge of the uses and misuses of statistics. He is not, of course, responsible for the flaws in ratiocination that may be contained in this paper.
4. This accounts in part for the male index of .403 for a set of 15 !Kung Bushman bands (data kindly provided and analysed (*per* method of Part I) by Lorna Marshall). As Mrs. Marshall (1960: 339) has pointed out, however, a number of !Kung males opt for permanent uxorilocality in their wives' natal bands.

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DISCUSSION

GARDNER: Perhaps at this point we should begin to consider the problem of lineality. Within our sample we have a case among the Birhor where a band formed around a patrilineal group is so fundamentally different from the material that we have been dealing with in this discussion that it should be used in comparison.

HELM: Yes, it might be interesting to see whether the Birhor who possessed lineages did have as many or more males, primary relatives or agnates, clustered together as did the Central Eskimo.

DUNNING: Before we can proceed, we will have to define lineality because, Dr. Williams, if I remember your comment, these bands were patrilineal, but they had no equivalence of siblings after the death of the father because father's brother does not have any special significance for brother's son.

WILLIAMS: Father's brother does have significance in making marriage arrangements for that person because in circumstances where the father is deceased father's brother may in fact help a man get a wife in the same manner as father would have. In this particular case my opinion is that everyone always tries to retain all his offspring, and the results are always the same. A man must give up his daughters in order to get brides for his sons.

DAMAS: I have often wondered whether some feature of the population structure favoured lineage development. Could there be contrasts in the population structure of our two regions? For instance, what is the average life span? How many children survive to adulthood? Are there male sibling groups, of any size that survive to adulthood, whose members married and had children?

WILLIAMS: I constructed a small age pyramid. Discounting those who died immediately after birth (for which time there are no data available), life expectancy is very short. About half of the infants die within the first 18 months. There are almost no males who live beyond 55 or 60.

There are a few surviving male sibling groups, but almost none of these have as many as three members.

DAMAS: These data are very interesting when compared to my own, for in my area I often found male sibling groups of three. I had been hypothesizing that lineage development was a result of having a large number of surviving siblings and the reason for the absence of lineages among the Eskimo could be that there were no blocks of surviving males available from which to form the nucleus of a lineage organization. In the course of fieldwork in the area, I learned that these siblings were still living but that they were often split among several bands. If there are three surviving siblings, it is rare that all three might be found in the same band. This applies to all three regions. There is potential in my populations for having fair-sized co-residential lineage groups. Rather large patrilocal blocks could have been recruited. But the principle of the lineage seems to come from somewhere else. There does not seem to be any relation between population

structure and the occurrence of lineages if the Birhor and Central Eskimo cases are taken into account.

This argument would concern chiefly localized lineages which Steward refers to in his evolutionary scheme. In the case of the Birhor, one lineage seems to encompass most or all of the band. In other cases lineages may override band boundaries and perhaps act as mechanisms of integration between bands.

WILLIAMS: The lineages may be localized in only one band among the Birhor because they are so small.

HELM (to Williams): In your paper, you mentioned that corporeal property is held by the lineage. How do the lineages define themselves with reference to this property?

WILLIAMS: It did not become obvious for a long time that there were lineages present among the Birhor, at least I did not want to call them lineages at first. A man may have a certain set of gods that he keeps in his house. His son will inherit these gods. The gods are moved with the habitation after due ceremony. The family may have the same sacred mountain. A woman who comes into the family cannot sleep in the house that has the gods unless she is very old, in which case she would take part in the ceremonies. Although at no time do they really say anything about the existence of lineages nor do they talk about them as such, they do have these symbols which can be divided if the lineage grows.

BICCHIERI: What happens in a case where a man has only a girl offspring and he tries to bring in a son-in-law? What happens to the lineage god under these circumstances?

WILLIAMS: There is a technique whereby the son-in-law is induced to adopt the lineage god, but usually it does not take place immediately. This raises the problem of matrilocality. I have never found a case where a man did worship a new god, the god of another group; consequently, matrilocality was rare.

DAMAS: Did the surrounding people have the same lineage names?

HELM: Is this lineage a family line or something more expansive than that? Is there a name for the lineage?

WILLIAMS: There is a word *paris* which I interpreted as being 'lineage.' On the other hand, the surrounding agricultural people also use the word *paris* regarding their clans. In their clan organization the name of the clan is retained even though one leaves the immediate area. That is not the case for the lineages of the Birhor.

The Sampal — the tribal village agriculturalists — have a set of clan and subclan names. There are also vestigial small tribes in the area which have the same names for their lineages. These same names are used for lineages among the Birhor.

McCLELLAN: Does that mean that they probably acquired the names through trading relationships with these other people?

WILLIAMS: Probably originally, but today most of the trade is with the Muslims rather than with the agriculturalists.

EGGAN: The Birhor appear to represent as close an approach to the patrilineal bands as we are likely to encounter. They have a rudimentary form of lineage organization; they are becoming conscious of the lineage, giving a name to it, but it still does not appear to have any corporate functions.

Julian Steward has prejudged a number of groups by calling them *patrilineal* bands. Some of these have a patrilineal element, being a lineage or part of a lineage, and some groups are not aware that they are lineages. The Birhor is one example of a lineage band because of the patrilocal residence and the consciousness of lineage as well as locality relationships.

When Julian Steward coined the term "patrilineal band" he had not concerned himself with this basic difference and grouped a number of types together. When Service shifted emphasis to the "patrilocal band," Steward replied ("American Anthropologist") that he did not see that the change was necessary. He still prefers to use "patrilineal band." He defines this unit in terms of patrilocal residence. He has put lineage in his definition of this band type, but then he includes groups that have formal lineage structure or clan structures like the Australian and Southern Californian groups, along with the Negrito groups like the Semang. If one consults the *Ethnographic Atlas*, all of these cases are suspect except the Australians and Southern Californians so far as possessing lineages is concerned. Steward explains the apparent absence of lineages in some of these bands on the basis that they were formerly exogamous but have broken down and have become mixed. All this suggests to me that it is important to make a distinction between those band organizations where lineage is not socially recognized and those bands where it is. It is only in the latter cases that Steward's patrilineal band really exists. If Steward's patrilineal band does not really exist or exists in limited number, it may mean that Service's patrilocal band may not exist either. There is, in addition, the problem of change from the one type to the other, and this is a problem of evolution which will be treated later in the Conference. In considering the two types together Service obscures the fact that they are two different kinds of organization, and for evolutionary significance he obscures the need to understand the process of moving from the one type to the other. I think it is worthwhile to separate the two types and not to call them either patrilineal or patrilocal bands until the groups are examined more closely because these terms prejudice the data.

We might ask Rogers and Dunning to tell us something about the Ojibwa groups which formerly possessed clans but seem to have been bilateral. What would be their mode of locality?

DUNNING: There is an emphasis on virilocality in the region which would be as strong as with the lineally-organized African groups with resident lineages. The term 'patrilineal,' however, bothers me in this regard, for to me 'patrilineal' means equivalence of siblings and structured sharing patterns. The Ojibwa that I know are not patrilineal in this sense, though they certainly recognize unilateral descent. They are not lineal, because 'lineal' implies presence of lineages that do not occur.

With regard to clans, they appear to be atrophied. In a different ecological range where more people could aggregate, the unilateral clans had totemic names, and they could operate as clans, but where they are now in a different ecological zone, the subarctic region, they cannot function that way and no substitution is possible. If one clan increases in population, others die out.

It is notable that although the clan system does not seem to function, the totemic nomenclature reinforces the kinship system, e.g., patrilineal descent and virilocal residence.

ROGERS: In the Round Lake region there is no lineage structure, but there is a high incidence of patrilocal residence within the bands, which may fall into a pattern similar to Damas's Central Eskimo situation. There is a small percentage of men moving out, but it is mainly the women who move out of the band at marriage.

DUNNING: Would you say that in your region the only identification which makes the organization more than a purely bilateral one is the weighting of virilocal residence? Is that the only aspect?

ROGERS: That would be the main feature, the tendency of the males to stay within their home band.

DAMAS (to Rogers): Do you have the tendency toward the patrilocal family? You seem to have mentioned this in your paper.

ROGERS: That is the ideal situation, but because of varying game resources there are many examples of fission within the band area. These occur over long periods of time. In a family with five or six boys who grow up to maturity in an area where the game resources may decline, some of them will have to move out. Some may go with their wives' families. They may stay within the band territory under these circumstances.

DUNNING: How do people feel about this? With the Pekangikum there is a unilateral unit in the sense that this is a unit of people, brothers, who are related to the father and the father's brother. This is reflected in the residence pattern but not just the residence pattern. The whole operation of cooperation is involved. When women come along there is a different behaviour pattern that obtains toward children; they may be cross cousins. Do these brothers you speak of who go off to live with their wives' people not show some sort of preference for virilocal residence?

ROGERS: There is a preference to stay with the father if he is alive, but again with the pressure of limited resources the sons may be forced to move away before his death. There are occasions of friction between father and son as well. In such cases they may prefer to go off to join their wives' people. In cases where the father is deceased, they may go with the father-in-law and he will become a father image. Many of the men will say "he is like a father to me."

EGGAN: In the case of the Pekangikum band in the pre-trading days when there were these smaller groups scattered around the country, to what extent did each of them approximate a patrilineal lineage, and when they merged was there a composite grouping of multiple lineages?

DUNNING: They seem to have closely approached lineages in these small groupings, though there was not much depth to them. The larger aggregations probably were multiple lineage groupings. The trapping camp certainly appears to have had a strong patrilocal emphasis.

EGGAN: Apparently these lineages could be strictly maintained because the composite group became endogamous.

WILLIAMS: Is there any incorporeal property which is inherited through kinship among these Ojibwa?

DUNNING: Not to my knowledge. Magical and conjuring drums are passed to chosen individuals on the death of the owner.

EGGAN: The totem itself might be incorporeal property, I should think.

DUNNING: Yes, but there is no choice in its ownership.

WILLIAMS: I wonder how substantial symbols must be before these units can be considered lineages.

DUNNING: It seems to me that a lineage is a corporate group. If there is no corporate group, the lineage does not exist.

WILLIAMS: But I assume people have various ways of recognizing this corporate nature. In some cases the recognition may not be very explicit.

DUNNING: For the Southern Ojibwa there are clan articles, robes, and equipment, which are passed down through the clans. Obviously the clan members will choose the individuals who will receive these objects, but they are definitely considered to be clan property. The drum that I mentioned previously was passed to a person who was considered to be a bright young man; the transmission did not follow clan lines.

GARDNER: So far we have dealt with patrilineal bands or ones that approach patrilineality. We should perhaps move to a consideration of matrilineal bands. There are five clear-cut hunting groups, which have 56 per cent or more of their subsistence based on hunting, that are uxori-local. These are the Nabesna, the Southern Ute, the Kiowa-Apache, the Cheyenne, and the Arapaho.

McKENNAN: I would add the Chandalar Kutchin to that number.

EGGAN: About one-half of the groups in the Plains are uxori-local. They have matrilocal residence, but they are not matrilineal. They have matrilocal extended families, but the bands are composite, and the society as a whole is bilateral.

The husband moves with his wife to a separate lodge because they can neither live in the same lodge nor even talk with the parents-in-law. The sons-in-law and the unmarried brothers cooperate in hunting, but the band comprises three or four hundred people — a whole series of these extended families, some of whom are related patrilineally, some of them matrilineally, and some of them are unrelated. The band is not thought of as a matrilineal group. There is no matrilineal descent. That is why I think the distinction

between matrilocal and matrilineal is an important one to make. The one matrilineal group, the Crow, has patrilocal residence.

WILLIAMS: Is it not true that the Pawnee were matrilineal, but that under the influence of buffalo hunting and the buffalo party they became patrilocal?

EGGAN: The Pawnee system is difficult to reconstruct in detail since there is so little information from the earlier periods. From Gene Weltfish's data, it appears that the residence pattern was mainly avunculocal so that these people do not fit into the Plains groups in that respect but are more closely associated with the Northwest Coast. A man would live with his uncle in a large matrilineally-oriented extended family. A most interesting case is that of the Omaha, who are strictly patrilineal so far as formal organization is concerned. There was a moiety division with all the classical relations associated with moieties, exogamy for instance, but their residence pattern was complex because in the spring the women characteristically lived with their daughters in the earth lodges. In other words the residence was matrilocal, and they owned a plot of land down at the bottom of the valley, which they cultivated for corn in the summer. After they planted the corn, they shifted to tipis and a camp circle. Residence became patrilocal for the summer hunts: a man and his wives camped with his clan mates. The older people and the smaller children remained at home. After the summer hunts the earth-lodge phase would begin. The corn from the fall harvest belonged to the women who would store it for the winter. During the winter some of the men went hunting with their wives and established hunting camps on a patrilocal basis. If one considers residence patterns as determinants of other features of social organization, such situations vastly complicate the picture.

McKENNAN: I am not sure how we can handle the Northwestern Athapaskans in this context. Some of them are definitely matrilineal in that they have matrilineal sibs and matrilocal residence for a period and later uxorilocal residence. The Upper Tanana are matrilineal, and their residence slant would certainly be toward the matrilocal side. The man serves a period of bride service, actually living in the same dwelling with the bride's parents, and then moves out when his family gets too large. This is borne out by the figures that I have on the birth places of husband and wife, and it is even more marked among the Chandalar Kutchin who have exactly the same system. The husband can come from within the band or from one of the other bands, depending on demand and supply. But they do marry within a larger group or within what Helm calls the "regional band." I would call this situation matrilineal and uxorilocal, and this pattern also applies to virtually all the Northwestern Athapaskans. I would be interested in learning what McClellan would say on this matter. Is it not true for the Copper River country at least?

McCLELLAN: It is difficult for me to reconstruct the aboriginal pattern. The ideal as it is expressed is a period of uxorilocal residence following which the husband returns with his wife to his own group, whether this is within the band or not. But one of the sons-in-law would stay on with the wife's parents. Their idea in marriage is to recruit someone to take

care of the wife's parents in their old age. Still I would not say that uxorilocality was a dominant pattern. It was matrilineality.

EGGAN: This gives lineages of at least three generations in depth for a considerable period of time. They may break around the edges. They could be called stem matrilineages. Did the lineages within the clan have names?

McCLELLAN: No, not to my knowledge. I suspect that among the Atna, who had large winter houses, there were probably localized lineages with traditions which were probably to be considered the incorporeal property of that lineage alone. But there was no visual symbolization of the lineage. With regard to the southernmost Southern Tutchone, they were definitely affected by the Tlingit, and they have sib and lineage paraphernalia and visual symbolization. The lineages have named houses and probably comprise corporate groups. The more northern Southern Tutchone have only matrilineal moiety organization, no named sibs or lineages.

EGGAN: This situation resembles the small patrilocal groupings within the patrilineal clan system in that they are small matrilocal groupings within matrilineal clans. The question is how would Service treat this situation?

McCLELLAN: Service says the clans come from the Northwest Coast. They are dismissed as having diffused from the chiefdom societies. I would take an extreme position—I think that matrilineal sib have considerable antiquity in the Athapaskan bands.

McKENNAN: I would agree with that view and feel that it would apply to the Koyukon, the Tanana, the Han, and the Tutchone. Osgood has reported the sib system for the Tanaina, and I think that it is aboriginal for the Kutchin as well, though Slobodin may not agree. That includes all the western Athapaskans except the Ingalik. They all have the system of matrilineal sibs. I am not certain of the residence patterns throughout the entire area.

EGGAN: It appears that the residence pattern is avunculocal except for the remote interior.

McCLELLAN: That seems to be the ideal mode, but today one typically finds groups of siblings who exchange marriage partners.

McKENNAN: Another point which is worthy of note here is that all these people have bifurcate collateral terminology instead of bifurcate merging as reported by Service. Hoijer demonstrated through linguistic evidence that bifurcate collateral terminology is the proto-Athapaskan type.

HELM: There is also the question of whether uxorilocality refers to household or to band.

McKENNAN: That is very true. As I look at my Upper Tanana monograph, I realize now that the matrilocality is not so clear as it should have

been because I was writing about marriages within each of the five bands that comprise the Upper Tanana and not marriages within the larger group.

HELM: In groups that are bilocal or bilateral, or at least not strongly virilocal, this temporary uxori-locality seems to occur frequently. It is variable in length but generally runs for two or three years. It makes possible a flexibility in residence. A man thus makes a contact in his wife's area and then returns to his own band where he is already acquainted with the people and locales. Manoeuvrability is possible between the two regions.

McCLELLAN: I had thought that I found a functional correlate in this as well. Although the Southern Tutchone and Atna did not know what they were doing, their initial uxori-locality really ensured that in times of starvation and famine they could quickly mobilize their forces, for the adult males would be familiar with two hunting territories.

HELM: That again implies that there was a substantial non-unilocal allowance. During periods of starvation a man was really entitled to go off to somebody else's territory.

SLOBODIN: Among the Eastern Kutchin certainly this initial uxori-locality prevails, and it was not felt to be a type of insurance, that is, learning another hunting area, because every competent person knew the entire area reasonably well.

McCLELLAN: Actually, the Tutchone and Atna always referred to their initial uxori-local residence pattern in terms of taking care of the old folks.

EGGAN: To fill out this picture with regard to the Southern Athapaskan groups, all of them—the Lipan, the Kiowa-Apache, the Chiricahua Apache, the Mescalero Apache, the Western Apache, and the Navajo—are matrilocal. The Western Apache have matrilineal lineages which are linked in a chain fashion in phratries. The Navajo have matrilineal lineages and clans and a phratry system of a more normal type so that in the southern extreme and the northern extreme of western North America there is matrilocal residence and clan organization. If one tries to explain this distribution in terms of diffusion, one must explain what has happened in the intervening area.

HELM: Might we not examine these residence patterns in terms of evolution? If there exists a base of favourable subsistence conditions and there is little mobility or fluidity, perhaps we can expect a linking through males in local groups. But if there is a temporary uxori-locality built in as a hedge, if there is present a certain predisposition to explore the female principle, it would be interesting to know just what is needed for complete uxori-locality.

LEACOCK: Why do we have to explore it? Are you not assuming virilocality to be a normal occurrence and that any deviance from it needs an explanation? An argument could equally well be made for uxori-locality as a normal situation. Assuming that it is not essential for a man to know the area that is being hunted, why should not the ties of the woman as a human being be considered as important? For instance, there is a widespread feeling

of the women that they should be with their mothers for the first child so that the older woman can act as midwife. In this regard there seems to be strong evidence for a matrilocal trend and female dominance in many activities among the Eastern Algonkian. At the same time, the missionaries and the traders appear to have discouraged such trends. In the *Jesuit Relations* the Jesuits spoke of the problems of getting the men to make the decisions. The men's decisions, however, were not binding because the wives did not agree. With regard to residence, it is possible to extract cases from *Jesuit Relations* of brothers living together but also a number of sons-in-law taking care of fathers-in-law. There was constant pressure from white agencies for patrilineal-patrilocal phrasing that may obscure an aboriginal situation.

HELM: I would amend my question to say that we must try to account for both uxorilocality and virilocality. Out of pure chance one might expect a 50/50 male/female balance residentially in regard to which spouse is the resident sponsor of the other. One then must explain why one group sorts out in the direction of female emphasis and another toward male emphasis.

I can then ask if a 'rule' of temporary uxorilocality may not arise in response to a particular subsistence base and may lead in time toward a further structuring unilineally and unilocally toward the female side. I am again starting from my impression that temporary uxorilocality seems to be an important feature of bilateral societies.

SLOBODIN: There is, however, the problem of assessing the uxorilocality in the context of the total residential situation. If, as may well be true among the Eastern Kutchin, there was a random distribution of residence pattern among uxorilocality, virilocality, neolocality, and temporary uxorilocality, which did exist in the area, the statistical emphasis would be on uxorilocality. There is the one added stress in that direction, and at any given period there were likely to be more families residing in a virilocal situation, but the total picture was one of considerable variability in residence.

PROBLEMS IN THE STUDY OF SHORT-TERM EVOLUTION IN SMALL GROUPS

By TOM McFEAT

RÉSUMÉ

Dans l'étude qui suit, je m'efforce d'examiner une notion qui peut s'avérer utile lorsqu'on analyse les petites modifications qui surviennent dans une culture, plutôt que les grands changements qui l'affectent. Envisageant les petites communautés, j'avancerai que la notion de *dérive* pourrait non seulement concourir à ce but, mais encore servir à identifier des changements directionnels plutôt que des modifications occasionnelles. Je soutiendrai plus loin que les changements directionnels demandent que nous considérions le changement en fonction des types de culture qui ont trait, d'une part, aux modes d'adaptation à l'environnement (types systématiques) et, d'autre part, aux modes d'intégration internes (types indiquant le style de vie).

C'est par le processus de l'évolution que s'accomplit, dans la communauté, la modification des relations qui existent entre les modes d'adaptation et les modes d'intégration. La *dérive* est le mécanisme à l'aide duquel on peut explorer chacune des possibilités de voir intervenir une telle modification.

This session has been designated 'evolution' rather than the innocuous 'culture change' with a view to focusing a global concept on short-term events in small communities. One use of the concept of evolution suggests the combining either in a common classification all the groups under study here or in some manner their ordering in relation to one another. It also suggests that communities and regions of communities achieve stages between points in time, in space, and in development, and these points, if ordered, demonstrate progress in the direction of higher cultural forms (Sahlins and Service 1960: 37) or cultures which evolve higher levels of integration (1960: 36).

TIME, SPACE, AND FORM: *Time*

(Traditionally the best method of studying specific evolution was to discover conditions where time was maximal, space was restricted, and form change was cumulative.)

In studying evolution, the greatest advantage is offered when time, space, and form are in such a relationship that time is lengthy, space is restricted, and form is changed in a consistent direction. Thus, Childe's most dramatic and satisfying examples of evolution come from such well stratified *tells* as Erech (Childe 1961: 59, 117); and Steward's six great civilizations exhibit great time depth, restricted spatial boundaries, and nonrepetitive and directional changes in form. Such studies, like motion pictures of flowers opening their petals to the light, are compelling; the process of change is emphasized rather than the static structure which underlies the form. Steward's *Cultural Causality and Law* (1955: 178-209) presents cases of "specific evolution"

(Sahlins and Service 1960: 13) in which new forms in *regionally restricted* cultures emerge from old ones as a result of "adaptive modification."

Contrasting studies are those where both the period of time and the units of space are extensive, and (because of the latter) environmental differences are of no particular significance, but where change nevertheless appears progressive. Morgan's and later Childe's general formulations of progressive stages developing from palaeolithic through mesolithic, neolithic, and urban or civilized societies are cases in point. Seen in this way, all evolution, if it occurs in the absence of diffusion from one society to another, would be specific evolution, creating parallel but separate forms. With the occurrence of diffusion, a pooling of adaptive modifications takes place and produces general evolution or 'progress.'

A strictly synchronic study of a community or region is indifferent to time sequences; if the study takes into account other communities in other regions, it is comparative; if not, it remains ethnographic. Thus an ethnographic study of a community or its institutions carries with it the assumptions that the community or its institutions will not change their structures, even though they may vary cyclically with seasons. It is therefore a matter of importance to recognize that such communities are not perceived as evolving, and it is equally important to recognize that in spite of this they may, in fact, be undergoing the kind of change which is, or leads toward, evolution. If they are changing thus, then something has escaped the investigator.

THE UNIT OF STUDY: *Space*

(The unit of study could not be the community since such units are prone to extinction while others spring into existence in complex form; it must be the regional culture.)

It would be desirable to observe a community for a thousand years,¹ but should the life span of a human be extended so as to make this possible, it is still not likely that he could induce the community under observation to follow suit. Communities, like cultures, to paraphrase Sapir, come and go. An example familiar to me is the Modified Basket Maker village Roberts called Shabikeschee (Roberts 1929) in Northwestern New Mexico, which underwent some change during its existence and then was abandoned; today it exists neither in the state it was in nor in its modified state. To consider a different example, the Hopi village of Hotevilla sprang into existence in 1906 following civil strife at Oraibi, twenty miles south in Arizona (Titiev 1944). Oraibi had been continuously inhabited since the thirteenth century, but in 1905 Hotevilla did not exist at all. Still, the principles of integration describing one community describe the other. A related phenomenon occurs in parts of the Eastern Subarctic in the various communities whose hunting territories were described by Speck and Eiseley (1942: 229, 231). In observing changes in hunting patterns, the authors noted that a local distinction between Great Hunters and Little Hunters had been created and that these, in turn, were distinguished by the Montagnais from what Speck and Eiseley called "proletarians"—or those who had abandoned hunting for stable situations in areas close to trading posts and agency offices. Fluid as the boundaries of these communities were, it is clear in retrospect that their life

appeared to be pouring like rivulets out of the taiga into the towns or, at least, their people were—men eventually, if reluctantly, following women and children. In 1960 many Montagnais could be located in a village called Maliotenam, several hundred miles south, southwest, and southeast of the furthest extent of the hunting territories of the previous generation. Neither the community nor the inhabitants were the same; even the number of family names had considerably increased. The relocated community now rested on no exploitable territory, was a great deal larger than any old trap-line outfit, and had changed complement. Like the heirloom axe that replaced both handle and head half a dozen times in the owner's lifetime, the reality of the older community rested mainly in recall; unlike this proverbial axe, however, it had not preserved its principles of operation.

Following examples of this sort, it is tempting to take the culturalogical position, and, like White, see evolution, independent of specific cultural forms and given geographical boundaries. If the only alternative to this position is to study the evolution of a given community the way may become a cul-de-sac. However, though one unit, such as a community, may disappear, another such related unit may continue to change; and if both disappear, it is not necessary to assume that an evolving culture must become extinct. It may be found that many communities, all dated differently, located at different places, and yet related, predate or succeed each other as though some, in failing to persist, were none the less able to provide new foundations and new principles of integration for those who survived. After all, Hotevilla succeeded Oraibi, and what has happened to the Montagnais communities during the last fifty years occurred probably a hundred years earlier (in a more or less similar manner) to other Algonkian communities. Those Montagnais communities situated along the St. Lawrence in the direction of Quebec City, now connected by road to Maliotenam, may have something significant to teach Maliotenam about the problems of its management.

While the first problem is to settle questions of time, space, form, and the appropriate unit of study, it is clear that we in particular are restricted regarding time. Even if it were possible, there is not much to be gained from an archaeological investigation in great time depth of all the communities under consideration here, since the primary concern of this Conference is very simple communities, that is, communities which are bands or band-like rather than tribes or tribe-like according to Service's definition (1964: 60-142). In other words, during their entire existence these communities did not significantly evolve; a look ahead far enough in time, say another thousand years, would probably reveal that all will have disappeared, for the universal evolution (or destruction) affecting mankind will have caught them up. Thus, we must be content with a brief time period; indeed, I think the ethnographic present and ethnohistorical past restrict the boundaries of the time within which we must work.

Regarding spatial extent, neither the world nor the community can be the unit of study; it must be something in between, like the archaeologists' *area co-tradition* or, to use a more familiar term, *culture*. If, then, Shabikeschee Village ceased to evolve, and Hotevilla sprang full-blown into its 1906 form, it is none the less true that Cibolan and Tusayan co-traditions, of which these communities were respectively a part, grew in a continuous way through

various identifiable time periods between A.D. 200 and A.D. 1900 in a restricted area of the Southwest United States. Anasazi culture most certainly evolved.

CORE CULTURE, LEVELS OF INTEGRATION, AND THE POVERTY OF ALTERNATIVES

(The concept of Levels of Integration in which different adaptive levels are described is not to be confused with evolution and, like the concept of adaptive specialization, does not indicate the nature or direction of change.)

Being ethnologists we are accustomed to our own instruments, not those of archaeologists. Instead of considering massive change, we are more inclined to specify situational or short-term dynamics. Instead of expressing depths of evolution, or the spreads of diffusion, or stages and distributions, we are more inclined to phrase the dynamics we observe in terms of culture contact, fields of interaction, adjustments and adaptations, or withdrawals and revitalizations. While one recognizes such events in the long run as but aspects of the total evolutionary process, one's focus is blurred in restricted time perspectives. It is very difficult to identify observed change not only as being directional, but also as working toward new levels of integration. The comparative observations made possible in this meeting are open neither to an archaeologist working through the successive strata of a site, nor to an ethnologist generalizing from a single community. But the purpose of this Conference is to benefit from comparative studies and to think in terms of types and processes of change. Thus, my task, while it cannot determine whether our discussion will disclose directional change, none the less remains to suggest concepts of evolutionary significance that apply to data occurring under the conditions of time, space, and form already specified. We are about to gaze at the stars through a microscope!

We at this Conference cannot approach evolution as a series of stages in universal progress (for then all that could be written is the evolution of the contact cultures which surround the groups represented here²). Nor can these groups seriously be studied only in the context of adaptive change or "adaptive specialization" (Sahlins and Service 1960: 14), as though the investigator had real knowledge of all the possible alternatives to action and therefore could judge that those observed were or were not evolutionary because they were adaptive or not adaptive. Sahlins and Service write, "In the specific context each adapted population is adequate, indeed superior, in its own incomparable way" (1960: 15). I doubt this proposition could be demonstrated or even tested in any of the specific cases under discussion, unless all adaptive alternatives had been removed from the realm of possibility.

Steward's Patrilineal Band is the perfectly adapted culture in the sense that, in theory at least, there are no alternatives to the pattern that was selected. It has a simple, clearly defined form (Steward 1955: 122) from which it varies only narrowly and then only temporarily. The patrilineal band is three things: a *culture type* which is also its *core culture* (1955: 122), a *mode of adaptation* to an environmental pattern, and a *level of integration*. The culture type, or form, need not be of concern here, but the

concept of *core* culture and adaptation should be, since core culture means adaptation. It is also necessary to determine if core culture also means specific evolution.³

Imagine several related patterns: an environmental pattern (natural integration), a settlement pattern (spatial integration), and a community pattern (social-cultural integration). If all of core culture, i.e., of the culture having reference to subsistence, is all the culture there is to describe, then the completely *isomorphic* culture can be observed. Therefore a culture in which the community pattern and the settlement patterns and all their aspects are specifically adapted to serve a function relative to the environmental pattern is an *isomorphic* culture. It is all core; since it is committed to the environment through its technology, it actually creates an adaptive model of the environment which is simultaneously a principle of organization for action in the community. If this were so in the case of the patrilineal band, it is because the conditions imposed by the environment result in a poverty of alternative adaptations. To quote Steward:

Ecological adaptations can be considered causative in the sense that a degree of inevitability in cultural adjustments is directly observable. (1955: 89)

This must be, then, the perfectly adapted culture since any other adaptation is impossible. It is judged impossible on empirical grounds, for groups under examination have all tried one or another deviation from the standard form and have found they were not workable.

Steward never stated that all bands are patrilineal bands; on the contrary, he also described family and composite bands and allowed the possibility of other types existing. He suggests that "there are probably several levels of sociocultural integration between the family and the folk society which should be distinguished" (1955: 54). Thus enters the notion of levels of integration of which certain institutions, called "basic" by Steward, are inherently related to such levels. In this preliminary discussion of levels of integration, Steward also develops the notion that certain basic institutions, themselves related to these levels, are inherently resistant to change — perhaps the whole of core culture — and become incorporated as components of socio-culture systems existing at more complex levels of integration. Examples presumably could be drawn from his major study (Steward 1955: 178–210).

If Steward is not a complete relativist (his notions of levels of integration and of basic pattern indicate he is not), neither is he one who, in embracing general evolution, has abandoned the idea of relativity, for unlike Sahlins and Service, Steward has been most particular in distinguishing between the concept of *levels of integration* and that of evolution. He writes:

The concept of levels of sociocultural integration . . . is simply a methodological tool for dealing with cultures of different degrees of complexity. It is not a conclusion about evolution. (1955: 52)

Thus, if the patrilineal band is these three things (a culture type, a mode of adaptation, and a level of integration), it most definitely cannot be an evolving culture. It is resistant to change; in larger scope it is static; and it represents the perfect unit for survival because it is locked in a stable environment to which it must adjust directly. It provides the kind of evidence which damned the efforts of some nineteenth century evolutionists.

BASIC AND SECONDARY PATTERNS:

Neomorphic Alternatives

(Steward recognized the existence of secondary and variable institutions as opposed to basic and constant ones, but his interest lies with the analysis of the latter, and he leaves the problem of the relationship between them open.)

It is regarded as a truism in anthropology that cultures, no matter how static they appear, are always undergoing change. However, the truism is not easily demonstrated unless adjustments to known specific stimuli of innovation or contact can be observed. The kind of thinking that creates difficulties in conceiving of communities as evolving probably originated the concept of the folk society. This model describes a small community with a primary division of labour and simple technology, animism, a kin base, and generational authority. The model describes a traditional society or range of communities without the mechanisms necessary for bringing about evolution. Therefore, when such societies or communities are observed undergoing change, they are thought of as acculturating rather than evolving.

But the folk society, at least that described by Redfield (1947), is not a structure; it is only a statistical generalization. As such, no more is known about what keeps it stable than about what fosters its change.

If analysis describing evolution requires a region rather than a community, then communities are seen as interacting with their environment; and since the object of concern is evolution, integration is also involved. All the criteria belonging to specific evolution must be included here: region, interaction with the environment, integration. Though we have a few good examples, the best ethnographic models available are provided by Steward, who is concerned with all three coordinates which, considered together, are synthesized in his concept of core culture as well as basic or primary institutions. However, it is a better model than folk culture.

The only problem with Steward's model lies in its emphasis on the community's control over stability, and it therefore provides no information on how, in the short run, evolutionary change is accommodated. Nevertheless the question can be asked, "Suppose some Patrilineal Band were in fact evolving—suppose this process were going on and everyone had missed it—what processes should be looked for that were missed?" This I believe, must be the next problem of the paper, and it inextricably involves the concept of basic institution.

It seems manifest in the restricted concept of core culture, or basic institutions, that the governing mechanisms in each community so described are conservative and apparently resistive to change. It is therefore surprising that Steward places the various cultures described in terms of such conservative institutions in an order not only of levels of integration, but also of *evolution* (Steward 1955: 178–210).

The first thing a closer examination of Steward's analysis reveals is that the Bushman and Australian cultures (both Patrilineal Bands) are not exclusively *isomorphic*. It is true only that Steward has considered them as such for purposes of his analysis. It follows that the Patrilineal Band is not all core culture, nor are its institutions only basic or primary. For Steward, core culture or basic institutions are the important nuclear aspects

of culture, not the only aspects. He argues that it is possible to demonstrate that culture is not a kind of organic whole or Gestalt (which an isomorphic culture would have to be if the environmental pattern were consistent) "... in which all parts are of equal importance and . . . equally fixed" (1955: 185). "Some features of culture," he writes, "are more basic and more fixed than others" (1955: 185), and he contrasts these more basic features with secondary ones, which, though somewhat consistent and functionally integrated with primary ones, "are more susceptible to fortuitous influences from inside or outside the culture (and) that change more readily . . . " (1955: 185).

Steward's interest lies in isolating institutions that are basic and constant rather than those that are secondary and variable (1955: 184). Although he recognizes both basic and secondary patterns, the latter do not appear from his view of organization to affect the former; for purposes of his analysis, only the basic patterns are considered. But it could also be argued that a concept which systematically relates basic to secondary patterns, or change-resistant to change-prone patterns, or systemic to style patterns, or static to dynamic patterns of culture is an essential point of departure for any short-term study of evolution. Otherwise, the result is a model in which structure is surrounded by randomness. This is also what separates the concept of levels of integration from the concept of evolution. If 'levels of integration' is a misleading concept for considering evolutionary processes on a regional, communal, short-term basis, it is because basic institutions have been *too* well isolated.

The question of evolution emerges through an investigation of Steward's approach in the following way:

1. Since short-term change is the subject of investigation, it is probable that secondary, variable patterns rather than the primary or basic ones will be observed to underlie manifest changes.
2. If the primary and secondary features or patterns are, in fact, connected and maintain some relationship in any specific case, then change that is observed in the secondary patterns must be affecting primary patterns; and, if the definitions are correct, there must be tension between the two since one set of patterns is structured statically, the other dynamically.
3. If the above statements are true, then a concept that will identify both primary and secondary patterns and throw light on the relationship between them is needed.

If primary features describe *isomorphic* process in culture, then secondary features, usually associated with uniqueness of a culture, describe what may conveniently be called *neomorphic* processes in culture; that is to say, a culture whose models are relatively free to develop in a unique or experimental way. The two might also be distinguished as patterns by following Kroeber's distinction between systemic and style patterns (1948: 313, 329).

If observation reveals that communities are ordered and if, at the same time, they are evolving, this must be because the variable (secondary) patterns are stabilized by the constant (basic) ones.

KROEBER'S ANALYSIS OF BASIC AND SECONDARY PATTERNS:

Controlled Change

(Kroeber was more concerned than Steward with the relationship between basic and secondary patterns, but regarded them as interpenetrating. With Richardson he explored this relationship in order to devise a method of separation.)

Kroeber was recurrently if not continuously concerned with basic and secondary patterns of culture; indeed, some of his later statements are reminiscent of Steward's. Stressing the primary importance of residence and hence of problems relating to environmental adaptations, Kroeber argued that aspects of social structure were likely to be secondary patterns rather than primary:

. . . while one must live somewhere, one can live without artificial exogamic groupings, descent reckoning or totems; co-residence necessarily brings associations which have social influence; just as one must have kin, but need not have clans. (1952: 217)

Though specifically related to problems of social structure, Kroeber's examples reflect his general interest in the basic-secondary relationship. To him formal social organization, especially of the Australian variety, represents experimentation, play, and variability ". . . which in turn argue relative intranscience" (1952: 217). Kroeber, like Steward, refers to the intricate, the structural, and the highly variable phenomena of culture as the secondary patterns: "They are in a sense epiphenomena to other, underlying phenomena . . ." (1952: 217). However, it is interest in these 'epiphenomena' that distinguishes Kroeber from Steward. Kroeber writes:

This would not in the least imply that they were regarded as trivial. . . A moment's reflection will show that the stable things in culture which everyone takes for granted are less likely to be in conflict or under strain, and will probably have only latent emotional interest connected with them. It is those things which are new and not yet worked out into a stable routine . . . the elements that differ or are beginning to differ from those adhered to . . . about which feeling ordinarily is strong. (1952: 217)

While Steward continued to be occupied by basic institutions, Kroeber's interest shifted toward the secondary ones.

They are nonetheless interesting on that account . . . but if they are mainly secondary elaborations and shifting experiments, it is important to recognize them as such, in contrast to the primary fundamentals. (1952: 217)

Kroeber tended to equate kinds of institutions with one or the other; thus, his "value-culture" and his "reality culture" which coincided with particular kinds of institutions certainly represented this primary-secondary quality to which he addressed himself. However, not to be restricted to calling certain *kinds* of institutions specifically primary or secondary, he writes:

Even in such basic subsistence domains as food and housing it is evident . . . that fashion plays a perceptible part. (1952: 218)

Kroeber's contribution to the study of this relationship which began in 1919 with his study of change in women's fashion was enlarged considerably in cooperation with Richardson in 1940. However, this investigation seems so tangential to problems of regional-communal-integral studies of evolution that to quote it in detail risks our bordering on irrelevance. Therefore, I will

cite only certain highlights to point out the methodological possibilities of the study.

It will be recalled that this study reliably measured six characteristics of Western European women's formal evening wear between 1787 and 1936 and more or less intermittently for the 150 years immediately prior. These measures (length and width of skirt, depth and width of waist, length and width of décolletage) varied through the period under investigation toward and away from maxima or minima. The fluctuations between either extreme produced half-cycles between 32 and 105 years (Kroeber 1952: 368), with an average of about 50 years. In other words, there was considerable regularity with the moving average for any given year increasing or decreasing along a scale toward one extreme or the other.

Possibly from the point of view of specific evolution, this is a remote event. Then again perhaps it is not evolution at all, but history, as Kroeber would probably assert, for he thought of specific evolution as history. However, there is something more to be considered, namely that Kroeber and Richardson were able to isolate a basic pattern by examining the fluctuations of secondary ones. Their major question ". . . under what circumstances, and why traits of fashion are relatively stable and unstable . . ." (1952: 359) was answered by the figures in two ways: first, measures were unstable when they reached a particular magnitude and stable as they reached another; second, they were apparently unstable (and therefore presumably reached the first set of magnitudes) in response to European or world-wide events, usually political in character and seemingly affecting the community.

The second reason for instability is not for the moment of interest, but the first is curious. It is apparent that instability of measures took place only with *particular kinds* of measures, and not necessarily with maximum ones; thus, minimum skirt length, maximum waist girth, and so on, tended to bring about the highest degree of variability for a given period. Therefore, during those periods where variability was at its *lowest*, Kroeber and Richardson isolated the basic pattern:

The best explanation that we are able to suggest for these phenomena is that of a basic pattern for women's dress style, toward which European culture of recent centuries has been tending as an ideal. This pattern comprises amplitude in most dimensions, scantness or medium value in others. As these proportions are achieved, there are equilibrium, relative stability, and low variability. At other times, most or all of the proportions are at the opposite extreme, which may be construed as one of strain, and variability rises high. This basic or ideal pattern, for Europe of the last two or three centuries, requires a skirt that is both full and long, a waist that is abnormally constricted but in nearly proper anatomical position, and décolletage that is ample both vertically and horizontally. (1952: 371)

In concluding the study Kroeber and Richardson suggest their method might be useful for other measures of "sociocultural unsettlement," and that it may serve "as a precedent for the more exact definition of other stylistic patterns . . ." (1952: 372). The stability of their measures applied to a relatively brief period (actually only 150 years) indicates that regularity in pattern-change can be manifested on a short-term basis, at the same time producing enough data to isolate both basic and secondary patterns.

Consistent with both Kroeber's and Steward's contention, this study showed that a basic pattern need not be found in one aspect of culture while a secondary pattern is found in another. On the contrary, it was shown with great clarity that there is a tendency for pattern variability to move directionally and in doing so sometimes also to reveal and expose the basic pattern. Whether useful or not here, the Kroeber and Richardson method of isolating basic patterns by exposing them through secondary or style patterns demonstrated a relationship that preoccupation with basic patterns alone could not discover.

ROBERTS AND EGGAN: *A cognitive Shift to Cultural Drift*

(The task of identifying the early stages of Drift was accomplished by Roberts; so minute were the shifts that they were unperceived locally. Eggan's study of Cultural Drift probably reflects an advanced communal and regional extension of the same process. Variation in patterning is part of all social life and does not necessarily stimulate notice and, much less, communication. But when it ceases to be random and takes on direction, then its departure from normative patterning is inevitable as are consequent reactions.)

In order to study 'drift,' it is necessary to distinguish integration as a level from integration as a process; I prefer the latter. 'Level,' or 'level of complexity,' is a useful concept, as Steward showed, for comparing principles of organization; under certain conditions, however (and these conditions affect the relevant studies), difficulties arise if there is no alternative but to approach integration as a stable 'level' being broken up by intervening processes of change. This problem does not occur when one examines the evidence from communities A, B, C...X from levels 1, 2, 3... n all belonging with the past. It is possible to tell on the basis of this evidence how bands became tribes and tribes became chiefdoms. Still, evolutionary theorists have not attempted to predict the next level of integration beyond that observed today; they do not tell us what 'community X' will be like. To do so requires anticipating unrealized innovations, foreseeing improvements on present innovations, and predicting the consequences of both. The concept of evolution has not been used as a predictive device and has provided no techniques for predicting the evolution of contemporary communities or cultures. All our cases fall into the same category: 'community X,' present but unaccounted for.

The communities under examination (Bushmen, Montagnais, BaMbuti, etc.) are designated bands, according to the static concept of integration. No one really knows 'where' they are going, or what they are evolving into. Will they become tribes, having been bands? Or will they become peasantries or even parts of town ghettos?

If contemporary living 'communities X' are seen as evolving, then integration must be a process rather than a state. It cannot be a stable plateau joined on either side by 'processes of change'; it must be a process of change itself. The idea of 'levels between which there are transitions' is, after all, arbitrary. I refer again to the sites in the Southwest which have undergone nearly two thousand years of growth. Site for site, which communities among them can be shown to have reached a plateau or level, and which others must be deemed transitional? Are 'Modified Basket Maker' or 'Developmental Pueblo' communities more transitional than 'Basket Maker'

or 'classical Pueblo' communities? Is it better not to think of Anasazi culture as stage-hopping but as a culture undergoing steady change at all the so-called levels of development? If my reasoning has been correct, there were no plateaus; change occurred not through the default of persons unsuccessfully trying to prevent it but under the guidance of communal leaders who directed it (McFeat 1960: 18).

Turning to the present and 'community X' or the communities of 'culture X,' the question arises, "Are they evolving or are they acculturating and therefore transforming directly into the likeness of a dominant group?" "Are they doing both?" With reference to such questions, Eggan's study of culture change in the Northern Philippines (1941) is of special interest, for I believe it surprised those who expected that everywhere communal acculturation is passive and direct in response to contacting groups. This was the first study that threw into question the simple assumption that if the dominant contact culture is Y, and the contacted culture is X, then X must become Y (a small event in the onward push of general evolution).

Since the groups Eggan studied—the Ilocano, Tinguian, Apayao, Bontoc, Ifugao, and Igorot—formed a unit of similar basic institutions, they were examined as a unit. The culture was more or less directly affected by contact with Hindu, Chinese, and Japanese influences from Java and Sumatra, and later by the Spanish (1941: 12). Thus, the various communities of the culture had been under acculturative influences from a variety of groups. Patterns of stability and change (the basic institutions and their tolerance for variability) were not simple matters of X becoming Y. Presumably the maintenance of cultural integrity depended in some measure upon the internal control of change preventing a haphazard result:

These people are organized in villages and their economic activities center around the cultivation of rice. Each participates in a common set of institutions to a certain extent, but there is considerable variation from one group to another. When this variation is examined more closely, it is found not to be haphazard, nor is it organized concentrically. As one goes from the interior down to the coast . . . a regular series of changes takes place in social, political, economic, and religious institutions, a series which has a definite direction. (1941: 13)

To this pattern of change Eggan applied the term *cultural drift*. It describes non-random directional change in culture, a process the character of which Eggan elaborates considerably in his 1963 paper. In the latter, he frequently interprets Herskovits (1948):

For Herskovits cultural drift represents the cumulative effect of small variations whose day-by-day effect is scarcely noticeable, but whose continuation results in long range directional changes in both the character and form of social life. (Eggan 1963: 347)

Both for Eggan and for Eggan interpreting Herskovits, drift is a process intrinsic to evolution, ". . . the theory of which was based initially on the steady and regular introduction of new elements" (1963: 348).

Eggan's study of cultural drift takes note of characteristics essential to the understanding of evolution. When the concept of drift is applicable to a regional examination of culture, culture is treated as a variant unit. Thus, his is a regional study of specific evolution as much as it is one of acculturation. All the abundant details reported in his paper suggest that the unit

undergoing change is that of interacting communities in which differentiation of function and of structure are taking place. The point that neo-evolutionists fail to convey is stated emphatically here: internal cultural diffusion is essential to evolution, even to specific evolution.

This study, like the 1963 paper, raises questions concerning the identification of behaviour or the immediate results of behaviour which assume drift patterns. I believe this question has been answered by a dramatic example of the most minute shifts taking place in culture in Roberts' *Three Navaho Households* (Roberts 1951).

The households Roberts studied were very closely related; their members interacted freely, yet even as families (despite their relationship) they constituted small distinctive group cultures (1951: 77). It may only be academic whether the term a *culture of communities* is used, as has been the case so far, or a *community of cultures* as Roberts describes it. In either case, internal diffusion marks its communal as well as its cultural characteristics; internal variations indicate the existence of distinctive units. In addition to the local distinctions which he established on the basis of an exhaustive culture element description, Roberts discovered the differences did not simply describe variations; he showed also that the variations possessed a directional quality; the three households could be ordered on a continuum of use or non-use of native or Euroamerican materials (1951: 80). In this manner data on the basic or primary institution (that is, the three families), on the degree of variation in habit patterns, and on the direction of the variation of these patterns emerge. Some of the most interesting findings in Roberts' study concern the variation itself and its direction. So slight were the directional differences in behaviour that they remained unnoticed among members of the three households. Roberts observes:

When the differences are brought together . . . there is evidence of a definite movement toward acculturation in HC (i.e., Household C) which is not apparent in HA.

This trend, neither totally unconscious nor totally conscious, is certainly non-random. It might well be termed "acognitive shift." (1951: 81)

He continues:

Perhaps because of the inconspicuous character, such shifts have been relatively neglected in the past. Nevertheless, many of the changes in culture must be of this type, and therefore additional investigations and analyses are merited. (1951: 82)

As Roberts' monograph illustrates, such phenomena can be identified only with the most detailed analyses; if changes taking place in the community are scarcely noticed by members themselves, then it is difficult to assume that they could be easily identified by an investigator.

The movement from directional shift in habit patterns to directional shift of culture patterns could describe a trend probably accompanied by the onset of individual awareness. Certainly, the Speck and Eiseley description of the native categories among Montagnais trappers differentiating 'Great Hunters' and 'Little Hunters' represents an arbitrary cultural and semantic cut between 'drifting' patterns, given the appearance of absolute alternatives. This drift must have begun in Labrador a century ago, perhaps describing a behavioral shift similar to and as subtle as that described by Roberts. It

represents the gradual withdrawal and reorientation of hunter-trappers from the limits of their territories. A year-by-year observation might have shown hunters travelling a shorter distance each season and decreasingly disposed to bringing their families as far in-country. With families more inclined to remain on the coast later in the season, the hunters became less willing to remain long periods in the bush. From the point of view of value-orientations, hunters began to look two ways, to the bush and the hunting life, and to the town with the school, the hospital, and wages. Their perceptions and, following these, their language then made discrete categories out of a continuum of drifting patterns. Perhaps this is the model of the process: a gentle, unperceived shift which with growing frequency and direction is not only perceived but labelled. A 'cut' is established semantically, perhaps through the process of focus. Finally, institutional change is culturally categorized in two basic patterns as discrete ways of life where, in fact, a whole continuum of pattern drift remains to join them. If this be the case, then ethnologists sometimes have been misled into believing that behaviour patterns that are variant to those specified by a semantic category are secondary and transitional, whereas the stable organization they are led to believe exists, or existed, is, in fact, nothing more than a point in a pattern of change. Such categories appear to bring movement to a halt and as a result are useful for native and ethnologist alike.⁴

Perhaps in the initial reactions to new behaviour patterns or in protestations over the loss of old ones, the first real clues about the relationships between basic and secondary patterns in institutional change, which have already been noted in Kroeber and Richardson's data may be discovered. Individual attention to change which is progressively increasing its range of variation seems to be an early clue to the drift that is occurring (whether cyclical, as it was with fashion change, or evolutionary), for the strain imposed upon basic by secondary patterns is necessarily noticed at some point. It is perhaps here, where the sensitive regions of activity Herskovits identified as focus exist, at the points where the basic and secondary patterns conflict, that evolution can be identified.

SOME CONCLUSIONS

(Adaptation to environment and integration of community or culture are the polar functional problems that have their form in basic and secondary patterns respectively. Drift is a mechanism which so regulates change as to permit experimentation while preventing disintegration.)

In illustrating some of the patterns implicit in drift, Kroeber and Richardson, and Roberts have put to good use the principle that culture in its selectivity produces remarkable 'natural' experiments. Eggan's study of drift demonstrates the result of the natural experiment from which he infers the pattern. Eggan also takes the matter into the realm of evolution not only because the change he describes is progressive, but also because it is regional in scope and concerned principally with change in the integration of communities. It is not a study of 'adaptive specializations,' although the problem of adaptation to an environment is clearly implied since the change in the organization of all communities is a response or set of responses to each other.

The drift to which Eggan refers clearly reflects tension existing between patterns of adaptation to the environment and integration of the community. Institutions that are organized in such a way as to adapt to the environment have already been referred to as the primary or basic ones or, taken together, the cultural *core*. But a much greater separation can be seen between *adaptive* institutions and *integrative* ones, and it is here perhaps that some elaboration is needed.

When Kroeber pointed out that people must "live somewhere" but not necessarily with particular residence patterns, or that co-residence implies kin but not necessarily unilineal kin, he was referring to the difference between primary and secondary patterns and tending to look upon the former as more or less of greater adaptive significance. Thus, when he described *systematic* patterns as stable, unchanging, utilitarian, multi-adaptive systems, he was reflecting in his terminology the character of primary institutions. In a passage quoted earlier, Kroeber acknowledged that in the fashion study conducted with Richardson he found that secondary patterns may accompany primary ones. His concept of *style* pattern reflects growth, change, uniqueness, and building upon a primary but self-limiting base. In point of fact, all institutions have the capacity, as Kroeber pointed out, of containing both an underlying change-resistant pattern and a radiating, or drifting, change-prone pattern; however, some institutions are more bound by the limitations of their environments than others. Subsistence-directed institutions have this character because the environmental pattern to which they are directed tends to be stable, or, if changing, forces change in the behaviour consistent with adaptation; this means that the basic pattern can be change-resistant only in the sense that, whatever changes are described, the patterns themselves are hard to alter since they are governed externally.

But integrative institutions are related in some consistent pattern to other types of institutions; their environment is internal rather than external since they are the 'epiphenomena' related to basic patterns. Therefore they tend to be the most experimental because of their relatively freer selection of more or less compatible basic institutions with which to interact. Consider the poetic, if partly fictitious, beauty of Benedict's Apollonian principle of integration: no institution of Zuni culture appeared to operate except as a reflection of its principle. More complex was the Directional Principle described by Cushing. Because of this principle of integration,

. . . no ceremonial is ever performed and no council ever held in which there is the least doubt as to the position which a member . . . shall occupy in it.
(1896: 370)

Anasazi integration is reflected in both ancient and modern settlement patterns. At least as far back as the very ancient Modified Basket Maker communities, villages were provided with ceremonial chambers, which had the same characteristics as modern *Kivas*: they were hidden-away, recessed back-rooms of houses; later, subterranean chambers; and finally, almost impossible to discover. Everywhere centres of integration have the look of the Ivory Tower; some of their activities are hidden from view, for those using them must have time to think and talk and concern themselves with their primary responsibility, that of maintaining the well-being of the community as a whole.

It is known that under certain conditions great innovators find themselves in a position as a result of nativistic or revitalization movements to organize action which has no apparent reference to adaptive reality. However, whenever this occurs, it appears to endanger the community; conceived as a response to the need for order, it brings little order. Such movements grow and change their directions speedily. Still it is not the speed itself that makes integration on a realistic basis impossible but rather that, in the absence of *drift*, no ordered relationship is possible between the simultaneously occurring needs to integrate the community and to adapt it to the environment.

Adaptive institutions, best expressed as *core* culture, tend to reflect isomorphic processes (i.e., toward perfect consistency between environmental patterns, settlement patterns, and community patterns). Their patterns tend to be basic, change resistant, yet not highly structured. *Integrative* institutions, which might be expressed as focal culture (borrowing from 'focus' rather than 'folk') reflect neomorphic processes (i.e., toward perfect internal consistency in selected aspects of the community). Their patterns are secondary, experimental, highly structured, and 'epiphenomenal' as Kroeber states. But they are capable of extensive scope and domain of influence in the community (e.g., the Apollonian 'principle') as well as, on the other hand, being sometimes quite apparently irrelevant. However, all institutions contain their adaptive (basic or systematic) pattern aspects and their integrative (secondary or style) aspects, as Kroeber clearly illustrated.

If our concern is with evolution, it must also therefore be with relationships between adaptive and integrative institutions in specific cultures. In the evolving culture the secondary variations around basic patterns cannot be random; these variations not only must be patterned but must be directional as well, in other words, *drifting*.

Drift is here interpreted as a process in change that is simultaneously of adaptive and of integrative relevance, for it is the pattern that emerges when problems of the relationship between adaptive (basic) and integrative (secondary) institutions arise as the result of change. It appears to reflect the community's control mechanisms, its governors of pace in change; it is the cautious way of getting things done, yet keeping them orderly, when the world is altering its form.

NOTES

1. Vogt (1960) suggests keeping a society under constant observation by several observers.
2. In this there is only one result expected: the disappearance of the cultures now under discussion and their final melding with the more dominant ones with which they are in contact. Their best possible adaptation, then, would only hasten this end; their past *failure* to adapt to the environment (i.e., their contact environment) has been part of the determining condition underlying their continued existence. Thus, that which is adapted in the sense of general evolution must be nonadaptive in specific evolution; in this case, then, there occurs a contradiction in terms.
3. Nevertheless, specific evolution is regional and therefore ecological, and although general or universal evolution is certainly of little significance here, problems arising from the relationship between regions and the communities that are within them still remain. I think the problem exists because regional units are also ecological units and culture-environment interaction and integration are involved. However, communities in regions not joined by political institutions are not integrated as units.

On the other hand while *integration* is a process central to the subject of specific evolution, it is a concept applicable to the community. With that problem in mind, I should like to consider Steward's recent work in which the processes of interaction with the environment (i.e., ecology) and integration are considered in relation to each other.

4. Lévi-Strauss (1953) warned against the failure to distinguish between 'home-made' models that are simply restatements of norms and those that are valid for the community.

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DISCUSSION

EGGAN: Should we begin the discussion by referring to Service and Sahlins's distinction between micro-evolution and macro-evolution? They felt that micro-evolution was a history of historical development and that macro-evolution was the grand philosophy of stages. If that is a legitimate statement, then it somewhat simplifies our problem. We can talk about either micro- or macro-evolutions, or both kinds. I am not sure that the distinction is a valid one, or even a useful one.

McFEAT: Perhaps it is not useful for the purpose of our discussion at this point. In general, I think Sahlins and Service have been wise to create the distinction between specific and general evolution, but they have left untouched the connections between the two processes. And it is not our purpose to try to connect them. Therefore, to attempt to recognize both micro and macro processes in our material—and I believe we must assume they are both there—would be confusing. If we are talking about or exploring the possibility of regular change in a band or group of bands, we are thinking in terms only of graded series of shifts as small events. If we are thinking of communication over extended areas—that is to say, diffusion—these small events can, in theory, carry us to the ends of the world and back. But this, I propose, we cannot do. Thus, the specific evolution of which we speak really ties us to an area, probably a geographical area, and thus establishes a unit which is neither a tiny event nor the world at large. The unit seems to be the community defined in two ways: (a) an integration of persons as its members in interactive time and space, and (b) a series of events affecting similar people in similar ways throughout a region.

EGGAN: It would either simplify it or confuse it.

LEACOCK: I think what we are really trying to do is to reconstruct the final stage of band society. There is a long gap between late band societies and primordial bands, though I think it is wise to limit our present discussion to the former area.

EGGAN: Discussion of bands in Service's *Primitive Social Organization* proceeds in the tradition of evolution on a grand scale. The hunters or band level people represent a stage which is broken down into patrilocal bands, composite bands, and anomalous bands. On the other hand, in the volume on evolution in which Service collaborated with Sahlins, another distinction is made without reference to such entities. If we discuss Service's theory, we have to be concerned with macro-evolution as well as micro-evolution, and also with their relationship.

HELM: I suppose one strategy is that we can consider some of the band societies that are more structured as opposed to those which have less structured organizations within the band. The question is, do we have significant differences in this regard? Do we, for instance, feel that lineage organization is somehow a more structured arrangement than that found among the Shoshoni?

WILLIAMS: There are fewer alternatives in lineage-oriented groups. In that sense they are more structured.

HELM: A good point. As another example, I would say that Service's concept of reciprocal band exogamy and virilocality with cross-cousin marriage is an elegant system, but it does not appear to be a simple one. I cannot imagine a group of Australopithecines working it out—it had to come at a later stage. We could perhaps start from our sample with the Central Shoshoni who have the least structured society and the most fluid, which resembles a "Street Corner Society" arrangement.

McCLELLAN (to Helm): In your discussion of methodology you talked about "seeding off in one direction or another" as if you saw the possibility of a society on the Shoshoni level becoming matrilineal or patrilineal.

HELM: That is right. I was assuming that a simple structure precedes a more complex one in the long run. Societies might also 'devolve' for a short-term period; for instance, matrilineality might be lost because of the particular area where the society may be.

I like Eggan's idea of 'drift' and the sorting out of alternatives, a drifting out to a more structured decision on one line and perhaps elaborating that decision because other alternatives have been rejected and lost.

DAMAS: Is this drifting a random process like, for instance, genetic drift, or is it a selective or adaptive process?

EGGAN: Drift is a concept that probably has multiple referents, but drift in general is usually thought of as moving in one direction. Otherwise, if there were random motion around a norm, there is no progression except perhaps breakdown.

There are two contradictory conceptions of evolution. On the one hand, we think of evolution as an achievement of more elaborate structure or more specializations. On the other hand, if we consider Steward's conception of cultural adaptation, it follows that movement from less adaptive to more adaptive structures is the mode of evolution. This may mean a breakdown from lineage to family level organization if it happens that the latter type permits survival in a particular environment. If one thinks in terms of an analogy to biological evolution, anything that is adaptive can be regarded as evolutionary. We tend on the other hand to depart from the biological analogy and use the progressive analogy and think in terms of evolution and 'devolution.' Under such circumstances the idea of stages does not seem applicable, because you cannot make the assumption that B represents a higher level than A and that it is at the same time more adaptive.

HELM: It is well to keep this in mind in considering that since it has been suggested that we cannot deal with the grand scheme of evolution here, the narrow range of societies that we are limited to cuts off perhaps 90 per cent of the social-cultural superstructure.

WILLIAMS: My assumption in accepting an invitation to a conference on band organization was that levels of sociocultural complexity were more or less accepted and that band organization was a stage we were going to work with, that the differences between macro- and micro-evolution have to do with adaptation—major or minor—that variability on this level would

be explainable in terms of these differences in adaptation. Probably this would be the most productive approach to evolution on this level.

DAMAS: In other words, you are saying that variability in our band structures is ultimately going to be explained in terms of variability in adaptation?

WILLIAMS: Yes. If the Shoshoni and the other groups we have talked of, in some places exchange brothers and sisters in marriage and in other places encourage brothers to stay together, there must be some reason for the variation.

HELM: But what concerns Damas is that he finds variation which does not look adaptive. I think that a society must be adaptive enough to survive but that there may be a range of alternative choices within that adaptiveness.

GARDNER: I must admit that on the basis of our discussion and some of the papers read here I have come to realize that this matter of adaptation is more complex than I had realized. I am particularly impressed by the number of social adaptations that are possible in the Central Eskimo area. If it is true, as most of us had assumed, that the Eskimo area is geographically one of the most difficult to live in and provides more limitations than most environments, then there can be no such close fit between precise environments and precise adaptations that are achieved. In this connection I am wondering how many people have been able to survive with poor adaptations. For example, the Yakut brought cattle up into Siberia and took them into their houses in the winter and fed them fish, even though cattle do not customarily eat fish. The cattle were weak when they led them to pasture in the spring, but they usually survived. The situation appears to have been similar in the early Norse period in Iceland. If people are able to survive with patterns of living like this, perhaps the degree of freedom that is allowed by an environment is considerable.

DAMAS: That is my own view. In addition, I also feel that many of these choices that are adopted are taken in a more or less random fashion under conditions of isolation.

SLOBODIN: Many philosophers of history and students of evolution have suggested that adaptability may have some negative relations to change and level of organization. The forms that become most adaptive to a niche in social or biological organisms are less likely to change or to evolve in a larger sense. It is the inadequacies and irritations, or what Childe calls in Marxist terms "contradictions in the system," which lead to change so that when we are considering the adaptations of forms of organization we might look to the difficulties in the adaptation, the inadequacies, rather than to the successes.

HELM: A point to remember in a macro-evolutionary perspective is that the harsh or limiting environments we are apt to associate with the regions exploited by hunters today did not characterize environmental conditions in the distant past. In the times before food production, many areas of the world offered hospitable environments, but the people who populated them

knew no other means of subsistence. I was impressed by the richness of some of the Neanderthal sites I visited in Europe compared to early North American Indian sites that I know. By present hunter-gatherer standards, large and perhaps relatively enduring settlements characterized the Upper Palaeolithic. Under such conditions, a general elaboration of social organization beyond what we can infer from present-day hunters may have obtained.

McFEAT: This brings up a point mentioned earlier that there seems to be an association of relatively highly structured hunting groups and fairly primitive agricultural groups coming together, the former contributing the technology and the latter the social organization. I think we should refer this generalization to recent work of J. M. Roberts.

GARDNER: With regard to this, if there were highly structured groups 40,000–50,000 years ago, I am sure that there were also some humble shellfish gatherers around at that time too; people were being pushed around, being forced into bad environments. There might have been refugee situations long before we can ever know. That is, it may have been true that in the Palaeolithic some people were marginal. Some of the populations that moved to the New World, which we think of as having been primordial, may really have been people who had been harassed in the Old World. They might have been retreating. Such factors vastly complicate our notions of what may have been primordial or pristinely primitive. We will have to wait until the archaeologists answer some of these questions for us.

At this point we can almost disregard macro-evolution as being irrelevant to our present interests. We are, rather, looking for a typology based on material which is more accessible to our direct investigation.

LEACOCK: Perhaps we should start at a level of immediate pre-contact bands. If we return to the question of typology, we might make a statement about the band level of sociocultural integration just prior to European contact. I do not think that we can talk about processes or evolution until we can agree on our typology. Certain features of band society have been found consistently, such as considerable flexibility, a lack of strong leadership, and a relative lack of status differentiation. Whatever the range of variation in these features, they still contrast with higher levels. The main variabilities we have discussed are in community patterns, in lineage patterns, in absolute size, and in movement. The last two are perhaps the least important typologically. As to the first two, agreement on a series of terms is the first step toward coming to agreement about which types occurred in the pre-contact period and which are post-contact. If we are to arrive at a frame within which to organize the material from all of the regions represented in this Conference, we must agree on the dimensions we consider to be important. Then we can begin to discuss causative factors or processes whereby one type moves to another.

TYPOLOGY

By FRED EGGAN, CHAIRMAN

RÉSUMÉ

Une typologie appropriée est indispensable à l'analyse du fonctionnement des bandes et à celle des facteurs qui commandent leur organisation. La révision de la typologie des bandes de Julian Steward, faite par Elman Service, se réfère aux données recueillies par June Helm, Eleanor Leacock, David Damas et d'autres encore, qui sont confrontées avec celles de Steward sur les Indiens Shoshonean et avec les études effectuées sur les tribus algonquines du Nord. Les conclusions qu'on peut en tirer suggèrent une variété de types de bandes beaucoup plus grande que ne l'admettent Steward et Service, ainsi qu'un mode de développement différent de celui que Service décrit.

Julian Steward has warned us against spending too much time on typology, but we face the paradox that typology is essential to furthering the proposed aims of this Conference: the analysis of process and the factors effecting band organization. To get beyond individual cases something must be said about typology. When problems of comparison are reached, individual cases must be reduced to more general types.

Interest in the typologies of bands began in 1936 with Julian Steward who was the first to attempt to bring order out of the chaos surrounding the concept of bands. To have attempted this task in American anthropology at that time was a remarkable achievement since he opposed the interest in historical problems by looking for regularities that occurred cross-culturally and by being interested in their explanation in terms of current sets of factors. He set up three, partly empirical and partly ideal, types of bands and defined their characteristics. He attempted to relate his types of patrilineal, matrilineal, and composite bands to one another primarily by way of cultural ecology and technology.

Elman Service is the most recent discussant of bands on this level. Although he retains Steward's two main types — the patrilineal band (renamed the patrilocal band) and the composite band — he relates them differently. For him, composite bands are the product of the breakdown of patrilocal bands. Service adds another category about which he says very little, the anomalous band, which includes the Shoshoni and the Eskimo. So far, I think the discussions have demonstrated that both these classifications are too simple, and with regard to Service perhaps rather naïve.

Eleanor Leacock has given a serviceable general definition of bands which emphasizes their minimal, interdependent, and viable qualities. If bands are examined from a structural standpoint, there are a number of factors to consider, which theoretically give several possibilities, permutations, and combinations. Empirically there seems to be a limited number of types, which I will examine mainly in reference to North American groups. In an

attempt to avoid discussing interrelations which will be dealt with during the session on evolution, I will try to define semi-empirically some of these types. The factors relevant here are descent, or perhaps, more broadly, filiation, in the British sense and in the sense that June Helm has been considering 'primary relationships'; marriage patterns, about which little has been said; and residence patterns, already debated at length. There are certain given facts here, particularly concerning the oscillation between small groups and larger ones. There seems to be a population ceiling for bands which we have not defined as yet, despite some discussion. There are certain cultural factors so far taken for granted: the kinds of kinship bonds or solidarity, common to all the groups represented here; and incest, apparently recognized by everybody but the Paliyans of South India, who seem marginal in this respect. It is possible either to work inductively, taking cases where certain common characteristics can be found, or to make models, i.e., our own or the ethnographer's, then deciding whether to be concerned about their degree of relation to reality. It is also feasible to discover the native model for a society, provided the people have conceptualized it, as have some Cheyenne who conceive of their kinship system 'as three horizontal lines,' i.e., the three generational lines that are the most important. Generally, typology combines inductive results and ideal types or models. I am not going to give a formal typology but will only sketch, with regard to the North American data, the types of situations that are of concern.

We might start with a group that we have not said too much about—the Shoshoni—one of the 'anomalous' groups that Service talked about and one that modified Steward's original typology. The Central Shoshoni of Nevada are completely bilateral, with absolutely no lineage emphasis. Men and women are roughly equal in status, although the women perhaps contribute more to subsistence.

Steward has noted that the basic unit here is the elementary or nuclear family, but he fails to emphasize sufficiently that family units are generally linked to other units through consanguineal and affinal ties. An enlarged family unit exists which may be composed of different nuclear families from time to time. Smaller family units may move around during much of the year, from one valley to another, depending on the variable food resources available. The sedentary winter 'villages' were composed of two to five nuclear families, or ten to fifteen people, although these villages occasionally were larger. The basic relationships within these 'micro-bands' were sibling links, and the winter 'villages' comprised siblings, male or female, and their spouses, in any or all combinations. Brother-sister exchange was a universally preferred pattern of marriage, though the actual percentage of occurrence is not known.

Shoshoni society can be conceptualized as a loose network of families who inhabited the whole area, over which information as to variable food surpluses and social gatherings could be transmitted. The social network was extended through nuclear family exogamy and random processes of courtship to the regional, or 'macro-band' level, represented by the Piñon Eaters, Squirrel Eaters, Seed Eaters, and similarly named regional groups, though these designations had little permanence. Beyond these local group-

ings the social network extended across the linguistic boundaries separating Shoshoni from Northern Paiute and Ute, as marginal communities intermarried and became bilingual. Warfare was generally absent within the Intermontane area, because of both a peaceful ethos and the general absence of resources worth fighting over.

Micro-bands at the multifamily level were more closely integrated through sister exchange, sororal polygyny, and various forms of fraternal polyandry, which formed 'double knots' in the social fabric. The larger local groups might hold gatherings in the spring and fall for communal rabbit or antelope drives, or for harvesting the erratic surpluses of piñon nuts and other products. Such groups were limited in size by the necessity of foot travel and the subsistence requirements at the gathering place.

In a few more favoured communities in the region along the Humboldt River and immediately south, a more adequate food supply allowed both a denser population and a more stable existence. There families could remain together long enough for their children to practise brother-sister exchange in marriage. Thus in four or five small adjacent localities cross-cousin marriage was sufficiently institutionalized to modify the kinship system. These regions were set off more sharply from the general kinship network, and the local groups were small bilateral bands based on cross-cousin marriage. Residence was generally matrilocal to begin with, but apparently was never stabilized one way or another.

On the northern and eastern margins the introduction of the horse resulted in an enlargement of the local group, which evolved into the composite band with some patrilineal emphases and new political developments. There is some argument about the types of bands that previously existed in this area. With regard to cross-cousin marriage, it is interesting to note that it was no longer possible to marry a cross-cousin but that one of the favourite marriage choices was marriage of pseudo cross-cousins. The pseudo cross-cousin is a mother's brother's wife's daughter by another marriage, or a father's sister's husband's daughter by a previous marriage, etc. Sociologically they are in the same position as cross-cousins, but biologically they are not related. This kind of marriage is found among the Northern Shoshoni and some eastern Shoshoni groups, and among the Comanche who moved out onto the plains around A.D. 1700.

The shift to pseudo cross-cousin marriage is intelligible in terms of the need for wider integration in the larger bands, though marriages still occurred within the extended family unit. The retention of this practice among the Comanche indicates that it is relatively old, antedating the introduction of the horse.

To the west among the Northern Paiutes in the Owens Valley, there is another kind of specialization on the central pattern. There the resources are much more plentiful, so that the population density reaches one person per two square miles. There was some tendency to plant wild grass seeds in favourite spots where streams spread out so that the crop was right at hand. More piñon nuts and game were available. Women controlled seed gathering, and there tended to be female ownership of these special plots; daughters therefore stayed with their mothers. Matrilocal residence was established in these areas, and Steward noted that the local groupings ap-

proximated a matrilineal band. This kind of development is also found among the Southern Paiute, mentioned by Peter Gardner yesterday. My own feeling is that this is the general situation underlying much of Anasazi or western Pueblo cultural development.

Regarding the Northern Naskapi, and especially the Barren Ground band, there appears to have been bilateral organization with no unilineal tendency. The kinship system was well adjusted to cross-cousin marriage which was either preferential or prescriptive. In spite of a probable patrilocal emphasis, residence was variable. I once ventured to describe their organization as a 'bilateral band based on cross-cousin marriage,' a description which also fits certain of the Central Shoshoni and perhaps some of the Central Californian groups.

Farther south the Montagnais are also bilateral, in that they have no formal patrilineal lineage emphasis. Cross-cousin marriage probably existed there at one time but shifted to more distant cousins or to non-relatives, perhaps under acculturative influence. Residence seems to have been variable, at least in severe ecological conditions and was 'balanced' in that it adjusted demographically to the number and sex of children in each family. There is a tendency toward patrilocal residence, with the possibility that the father-son relationship is more important to trapping than that of father-in-law-son-in-law, other things being equal. For the Central Shoshoni, there is a winter concentration and summer dispersal, but in the Montagnais-Naskapi area there is summer aggregation of macro-bands of some sort, and winter dispersal with two or three families per group being the ideal size. This condition is probably related to the exigencies of trapping, since the chance of surviving accidents with two males in the field is better than for a lone hunter.

Among the Northern Ojibwa in the Berens River area, the groups are formally patrilineal in descent, the kinship system is bilateral, marriage tends to be contracted between cross-cousins, and residence is indicated as patrilocal by preference. Farther south, in southern Canada and on the border, residence seems to be rather variable, and there is a family nucleus based upon father-son and brother-brother relationships that is fairly important. The network of the wider society is maintained by a superimposed clan grid that regulates marriage. Relations within the community are handled by kinship rules and regulations, but when travelling in more distant regions, hospitality, aid, and assistance all operate through the clan-phratry system which extends almost to the limits of the Ojibwa social system. The clan system becomes more and more 'corporate' farther south.

In E. S. Rogers' Round Lake Ojibwa area to the north, the situation is very similar to that of the Crees. There is no formal lineage or clan organization. Kinship relations are basically bilateral with regard to the organization of hunting and trapping and tend toward a patrilocal emphasis. There, it is possible to see the effects of the trading post. R. W. Dunning, in particular, gives an excellent description of what happened to relatively small bands in the Berens River region whose members intermarried with adjacent bands. With the trading post there is a much larger group and a general increase in the population density. Marriage still takes place between cross-cousins, but the range is wider, and as the bands become more com-

posite, they become more endogamous. If this process were continued to its ultimate conclusion, these bands would become small tribes or subtribes.

The North-Central Athapaskans seem to be largely bilateral and to be operating in terms of sibling linkages, that is, sets of siblings and their children. Marriage patterns are variable, and in a few places there seems to be evidence of cross-cousin marriage, not mentioned in the reports that have been given here. Residence may be weighted in the direction of patrilocality. There are two types of bands, macro-bands and micro-bands. The native models for these are perhaps clearer than the ethnographic ones regarding local bands. I was surprised to learn from the interesting material presented on the Northwestern Athapaskans that the matrilineal clan system extends farther into the interior than I had thought. Matrilocal residence, and in some cases avunculocal residence also exist. The partial matrilineages, which exist within a framework of matrilineal clans, seem to comprise bands of some sort.

On the basis of David Damas's description of the Eskimo, the variety of band forms that exist in the Central area and their relative adjustment to a great variety of micro-ecological conditions become evident. If the survey is extended to the Bering Strait Eskimo, and even further to the Pacific Eskimo, we find regions such as Nunivak and St. Lawrence Island where there are patrilineages. There is also one reference that suggests that the Pacific Eskimo closest to the Tanaina may have had matrilineal moieties. There are a number of specializations present which are perhaps related to the denser settlement patterns, to a richer supply of food and greater degree of stability, as well as to possible contacts with other groups.

The African and Indian cases are not so clear in my mind. One of the Indian cases, as well as some of the African ones, was an excellent example of patrilineal bands. These two areas also provided the most extreme cases of fluid bands that are near one end of a continuum of band types.

It might be noted, in conclusion, that this brief survey gives a picture of a much greater variety of band types than either Steward or Service allows, and if we looked at other areas of the world from the point of view of bands I am sure that a number of other combinations would be found. There are a limited number of types, even if six or eight basic criteria are chosen and all the combinations and permutations calculated. What to call these is not so important as recognizing that these variations exist. Some of them might ultimately be treated as subtypes of the more basic types, and thereby return to a simpler dichotomy or trichotomy of band types. I think it is more important in the interim to consider this full complement of variants, because only in that way will it be possible to examine the full range of factors affecting them. Our task in the next part of the Conference is to try to generalize these factors in terms of process.

DISCUSSION

DAMAS: I think that if I were to extend the model that Eggan has presented for the Shoshoni to the Eskimo groups I discussed, which have been likened to the Shoshoni with respect to level of integration, the Shoshoni can be seen to represent a much less regular situation in that family units associated alternately from year to year with other family units.

EGGAN: They may have. This picture was built on the basis of informant recall. Steward cites a few statements to substantiate this, such as: "Well I lived here with my brother for three or four years and then when I was older I lived with another relative." Steward's argument is that every valley was alike, and if an individual were harvesting piñon nuts in a valley where gathering was successful, a number of people would aggregate there for the harvest. This would not serve to spread out the network very widely. The range of movement was perhaps determined to a large extent by the distance that the nuts could be moved in storage.

DAMAS: It appears from your account of the Shoshoni that the micro-bands, or local bands in Helm terms, seldom joined all together into the macro-band or the regional band. There was alternate aggregation of smaller units. This contrasts to the Central Eskimo situation where, because of a peculiar ecological situation more than anything else, complete aggregation of the regional band was more frequent.

EGGAN: Yes, I am sure that was true.

LEACOCK: The interlocking network that Eggan has spoken of among the Shoshoni represents an important concept, one which I think may apply to the Algonkian area as well. As I try to reconstruct the Montagnais-Naskapi band for the pre-contact era, I wonder how stable any group could be before large numbers could support themselves during the summer on beans and flour bought at the trading post. Further, the scattering of people in the difficult winter season and the ease of crossing over an interior watershed to come out of the woods in the spring at a different place from where one went in, make it difficult to neatly circumscribe bands and their territories. Perhaps the Naskapi were similar to the Shoshoni, for whom this really could not be accomplished.

EGGAN: There is also the problem of boundaries on the edge of the Northern Paiute and the Shoshoni. The cultural characteristics differ, but the social structural characteristics do not.

SLOBODIN: If one looks back to before the days of the fur trade in my area, one must think of people who were spread out on the land, that is, not gathered at a post. Then if one thinks of the term 'interlocking network' of kinship in that context, it suggests a network of wires, so to speak, which people then can use. That is not how kinship seems to operate in my experience. It is not a 'given' in that sense, but is rather created by people's interaction.

HELM: Is it not created by the fact that an individual is born of such and such a man and woman and therefore will have an uncle somewhere? The social tie is activated along the wire.

SLOBODIN: Active kinship is, I think, created by interaction, and my experience with the Peel River Kutchin is that kinship that amounts to anything is oriented by interests and advantages and, as I suggested in my paper, by important people. The Kutchin say that rich people have many relatives, and poor people do not have any, but such situations can change in the course of a lifetime. Of course, everyone is continually jockeying for advantage in this regard. I do not see kinship as if it were some continuing natural phenomenon that could be used, but rather as the initiating and maintaining of interaction patterns. The point here is that the Peel River Kutchin conceived of their having cross-cousin marriage, but I did not recognize it as such. People who were marriage partners were ones that came to be called brothers and sisters and cross-cousins for their own purposes. Of course, they were people who were related as pseudo cross-cousins, or cross-second or -third cousins, but there was by no means any fixed pattern of cross-cousin marriage.

HELM: With regard to the marriage practices of the Shoshoni as reviewed by Eggan, the emphasis was on the brother-sister exchange. I pointed out a similar situation in Damas's chart of the Killinirmiut. When he referred to an example of cousin marriage, he could also have been talking about brother-sister exchange in that case.

EGGAN: That is true for one generation, and it is the beginning of cross-cousin marriage.

HELM: However, most of these marriages are brother-sister exchanges without being also cross-cousin marriages.

DAMAS: How would we expect this marriage practice to affect a terminology? I thought I had a correlation worked out between brother-sister exchange marriage and kinship terms on the first ascending generation for some of the Central Eskimo groups.

EGGAN: Most of the Great Basin Indians have bifurcate collateral terminology. That is, FB is a separate term from MB, and also there are some self-reciprocal terms, since the sororate, the levirate, and sororal polygyny also occur. All these factors are ones that we expect to be correlated with terminology, but if correlation is looked for on a world-basis it is very low.

DAMAS: For the Copper Eskimo I had worked out a correlation or consistency between brother-sister exchange marriage and the terminology, but in practice I found only a few cases of that kind of marriage in the whole population. This is one difficulty in trying to explain existing terminologies after unusual marriage practices; these marriages do not occur very often.

EGGAN: It may well be that the anomalous bands of Service, the Shoshoni and the Eskimo, may be basic to the whole North American area, and the

anomaly would be the appearance of the patrilineal band. On the basis of our limited survey of the Yukon area, Africa, and India, it appears that it is only on the margins that specializations began to appear. In the central areas there do not seem to be any real cases of descent. The relations between generations in those regions may be by filiation rather than by descent lines or descent groups.

BICCHIERI'S SCHEME OF BAND CLASSIFICATION

EGGAN: At this point I will call on Bicchieri who has devised a scheme for classifying bands.

BICCHIERI: It occurred to me that if we consider morphologically the data given from North America as well as from Africa and India, there seems to be a possibility of a five-point breakdown along a continuum of band types. I tentatively use the following terms: (1) dispersed, (2) dispersed-cohesive, (3) cohesive, (4) dispersed-agglomerative, (5) agglomerative.

By *dispersed* I mean groups that are broken down to the nodal family level; nuclear and stem-nuclear families, dispersed most of the time and joined together in a rather informal and random manner, at what we have been regarding as the band level. The Shoshoni are our example of the *dispersed*.

For *dispersed-cohesive* there is a closer approach to cohesiveness. As representative of this category I would use the !Kung, to signify groups which by necessity disperse at the nodal stem nuclear family level but which do come together in a more formalized or regular manner, much more so than do the *dispersed*. The *dispersed-cohesive* may have reference to a territorial point such as a water-hole or something similar. There might be inheritance of this territory.

By *cohesive* I would cite such groups as the net-hunting BaMbuti and perhaps the groups from India discussed here. At this level there is a consistently united group with fluidity but not much variation in group size. These groups stay together most of the time; dispersal would be regarded as being an anomalous situation.

The Central Eskimo groups or the Kaska would be representatives of the *dispersed-agglomerative* type. They would be groups of micro-bands which aggregated into the macro-band, by contrast to the first three types which would split part of the time into smaller units than the micro-bands. In other words, the *dispersed-agglomerative* maintain units larger than merely nuclear families.

For the *agglomerative* type, the emphasis would be more on the macro-band level. Perhaps Slobodin's Peel River Kutchin, with their larger aggregations, would be an example of this type.

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DAMAS: Referring to your example of the Central Eskimo as *dispersed-agglomerative*, I would say the dispersed segments were usually but not always larger than the nuclear family.

To make this system consistent, the fifth category, *agglomerative*, should consist of macro-bands which stayed together most of the time. This would

give correspondence with (3) *cohesive*. I am not sure that we have any of the latter societies in the ethnographic sample presented by members of the Conference.

BICCHIERI: I suppose that we can talk about the Northwest Coast Indians as *agglomerative* if we are not concerned with the extent of mobility. I have no reference to mobility in this scheme.

DAMAS: The North Alaskan Eskimo of the coastal area and the Bering Sea Eskimo might be other examples of the *agglomerative* type.

HELM: If such groups as these are to be considered in the scheme, the *agglomerative* end of the continuum becomes quite sedentary.

MARSHALL: With regard to the *dispersed-cohesive* category, must the bands break down to the nuclear family alone? The !Kung whom you have used in this type usually have the extended families as the smallest unit. These extended families do not, however, forage for themselves. Two or three of them usually stay together.

BICCHIERI: I wanted those types above the *cohesive* (4) and (5) to refer to micro-bands which joined together, rather than smaller groups merging — whether they broke down at the familial level or at something larger.

LEACOCK: Are the five types to be considered a continuum? They appear to form a 'V.'

BICCHIERI: I agree that some modification is in order. By replacing *dispersed-agglomerative* with *cohesive-agglomerative*, which would indicate the dispersed groups as micro-bands, there would be more consistency to the system. This would give the following pattern: (1) dispersed, (2) dispersed-cohesive, (3) cohesive, (4) cohesive-agglomerative, (5) agglomerative.

A contrast would be highlighted between (3) and (5) in that *cohesive* implies close interaction within a group, and *agglomerative* implies a looser interaction within the largest aggregation. Such situations would be more typical of settled villages than of bands. As Leacock has suggested, size would be important in this context in that the *cohesive* bands might have a size range of 25 to 100 people, a size which would allow close interaction, whereas in the *agglomerative* bands groups would probably be too large to admit such close interaction. At the *cohesive-agglomerative* level (4) dispersal occurs at the *cohesive* level and then alternates with agglomeration. At the *agglomerative* level there is not the physical possibility of intensive interaction among all the members of the band.

HELM: Perhaps we can make another distinction between the *dispersed* and the *cohesive*. In the *dispersed* category where there is dispersal into small groups, which occurs probably in most cases for much of the year, the composition of the small groups is variable. For the *cohesive* there would be more regularity in the make-up of the small groups. They would tend to be unilateral or unilineal, that is, one rule of linkage would operate rather than a number of alternatives.

BICCHIERI: Another way of describing this situation would be a statement elicited from the people themselves. Members of a *cohesive* band could say "The time is up, we've got to merge into a larger group."

LEACOCK: Are there not several continua involved? One is a dispersed-to-cohesive continuum, and another relates to the size of the units. In place of a single scheme, I think we ought to determine this significant continuum.

McCLELLAN: Perhaps that is true in the sense that we have confused a quantitative criterion — size of groups with a basically qualitative one — degree of dispersal.

LEACOCK: Our strategy might be to pool dimensions that should be defined in a disciplined way, and to cease talking in terms of our individual groups. If we can agree on a series of dimensions that we feel are significant, starting with those Bicchieri has raised, then we might arrive at a terminology that we can all accept.

SUMMARY DISCUSSION I

DAMAS: At this point it should be noted that in the preceding session devoted to evolution, Leacock suggested that progress in considering evolutionary processes could be made only if a more sophisticated typology of bands were developed. Bicchieri has presented a scheme, which has been briefly commented upon. Perhaps this scheme might be the logical starting point for us in our attempts to draw together the earlier discussions into a concluding statement.

HELM: In Bicchieri's agglomerative type, certain *community patterns* occur because of the agglomerated *settlement pattern* that exists. Perhaps it would be well to separate these two types of pattern and then see whether there is an inevitable nexus between the two.

In addition to these two concepts, it would be well to consider another dimension. The *exploitative pattern*, I believe this is Steward's phrasing, would seem to be appropriately related to the other two. The *exploitative pattern* involves the ways by which a group gains the material goods of life through the agency of a given technology. I suggest that *exploitative pattern* must play a strong role in structuring *settlement pattern*. Settlement arrangements that do not allow the people to exploit the resources effectively enough to survive cannot endure. The *exploitative pattern* forms the base for the *settlement pattern*.

EXPLOITATIVE PATTERN

ROGERS: Exploitative pattern would, of course, be intimately linked with the environment. There is a question of whether we should consider environment separately from exploitative pattern or whether we should put them into the same category.

McFEAT: I think that the term 'exploitative pattern' should subsume the environmental-technological relationship.

If we seek to employ 'exploitative pattern' as one of the axes of tools for typologizing bands, what subcategories would we wish to include? Perhaps one such would be the *hunting-gathering-fishing ratio*.

EGGAN: In the Yukon area, where about half the related groups were primarily fishing and the other half primarily hunting, might the difference be related to the relative abundance of fish? It would be interesting to know if there were concomitant variations under these circumstances in other aspects of the society. We could generalize one area in this respect and then try out the generalizations in other areas.

WILLIAMS: Could we not set up another continuum along the axis of the variability of resources exploited? We might assume that the gathering of wild acorns would show no more variability of resources than salmon fishing and might have equivalent effects on bands. On the other hand, rabbit populations could be more variable, and the annual migration routes and numbers of caribou could be even more variable and could result in differences in band organization.

LEACOCK: There seem to be two factors involved here. First, there is the *hunting-gathering-fishing ratio*. Secondly, there is the stability or predictability of the resources, i.e., whether there is a constant but perhaps low level of food supply, or a situation of periodic famines.

BICCHIERI: I have tried to work out a system for classifying dependence on hunting and gathering. I included fishing with hunting, a decision which might be controversial. I divided hunting and gathering each into four categories. The first of these was 'dependence on produce.' This category was in turn divided as follows: with regard to hunting (a) non-exploited (no hunting), (b) secondary dependence, (c) critical dependence. Critical dependence would refer to situations where lack of hunting at any one season could have endangered the lives of the people. If it would have simply caused inconvenience, then I would have classed hunting as being of secondary dependence. If a group did not hunt or hunted only minimally, I would class them as non-exploited.

The second category concerns distribution in space, that is, whether the game was dispersed or concentrated. The third category refers to distribution in time, whether the game supply was continuous or cyclical, and the fourth category is the number of items on which the people were dependent.

I apply the same categories to gathering. After calculating each society in terms of their relative dependence on hunting and gathering, I gave a score for each of the first three subdivisions of the first category: zero for the first, two for the second, four for the third. From this I would derive a hunting and gathering score for criticalness of dependence. In addition, I would use the other categories. For instance, if the resource is critical and also seasonally cyclical, I would give a score of 'critical plus' for that particular society.

HELM: There may be resources that are always present but which are still seasonal resources, e.g., fish that cannot be reached through deep ice, but the critical factor may be whether the resources that are expected in a cyclical pattern appear or fail to appear altogether in a given year.

BICCHIERI: Yes, I would put such factors into another series of categories, which I evaluated in terms of dependability. In addition, there are to be considered problems of the permissiveness or interdictiveness of the habitat.

LEACOCK: Perhaps most or all of what Bicchieri has referred to could be subsumed under a major category called *character of resources*, including dependability, concentration, cyclical character, special features, and so on.

McCLELLAN: It occurs to me that such a classification might be too broad. We have been thinking mainly in terms of hunting, fishing, and gathering. Are there not also climatic factors to be included under resources?

BICCHIERI: There would, indeed, be a number of subcategories. Water distribution, environmental factors such as blizzards which would be likely to impede the exploitation of resources, the dependability of resources, and also the relationships toward surrounding peoples, that is, competition over resources.

ROGERS: We should also consider resources for technological production: materials available for construction of tools, etc.

EGGAN: It is important also to consider surplus of resources at certain times. In a general evolutionary process the problem of surplus is important, and surplus also tends to tie groups to a locality.

BICCHIERI: I have put surplus under *control of resources*. That category would include storage and preservation techniques. I give a ranking according to the length of time through which the control lasts.

HELM: But would *control of resources* be limited only to surplus?

BICCHIERI: No, I would include subcategories under that item.

McKENNAN: The next category that we could consider seems to me to be an obvious one—*technology*.

HELM: Would *technology* not be subsumed under *resources*?

EGGAN: I wonder whether *technology* should not be made a separate category? In Steward's conception, technology, environment, and society are the three major corners, and I think that we should keep them separate.

HELM: Need we really consider technologies at all? The American Indians, by and large, had the same basic tool technology despite the wide range of environments that are exploited. How would we classify technologies if they are all basically simple?

BICCHIERI: We can make a differentiation between techniques and technologies. Techniques may be more or less refined or elaborate on the base of a simple technology.

ROGERS: I think that technology varies tremendously. The Eskimo, for instance, have evolved very highly with regard to technology in contrast to the Subarctic Indians who in some areas use the bow and arrow and the net.

EGGAN: If the Eskimo are regarded as having a more elaborate technology than the Northern Athapaskans, for instance, how can we characterize that difference?

HELM: One example might be that the Eskimo use a detachable harpoon head for hunting seals. The Athapaskans do not have seals in their habitat and therefore do not have the detachable harpoon head.

DAMAS: I believe that the Eskimo have a more elaborate inventory of caribou hunting tools and perhaps more refined techniques of hunting caribou.

McKENNAN: The presence or absence of the fish net seems to me to be a very important technological feature. The different ways in which fish and caribou are taken also vary within the Northern Athapaskan area.

HELM: With regard to the constructions for caribou drives found by McKennan in the Yukon area but not in the Mackenzie area, my guess is that the way the caribou move in the flat forests of the Mackenzie obviates the need for such structures.

McKENNAN: I think you are probably right in this matter. There are different environmental conditions present.

DAMAS: This would be a case, then, where different environments influence the techniques used in hunting the same type of game animal in two regions.

HELM: I suggest that we will know better how to define exploitative pattern once we know what in the environment is relevant to social and cultural life—after we have outlined the settlement and community patterns.

McFEAT: At this point we could summarize 'exploitative pattern' as we have discussed it so far: (1) hunting-gathering-fishing ratio; (2) character of resources; (3) control of critical resources; (4) technology.

SETTLEMENT PATTERN

GARDNER: If we are going to discuss settlement pattern, I should like to suggest criteria in addition to dispersal or cohesiveness that we might consider. I think it is relevant whether or not there is seasonal fluctuation. It is relevant what the minimal-maximal group averages are. It would also be helpful if we had some index of turnover or stability in group membership from year to year. Given these categories, I think we might approach a population study and an interrelationship study that could be used for a typology aimed at establishing relationships between population and other cultural features. There may be other categories to consider as well. We will need more than one axis in our typology.

LEACOCK: With regard to the category 'minimal and maximal aggregates,' there are two separate dimensions—ranges in sizes of minimal units and ranges in sizes of maximal units.

GARDNER: It is complicated even more in some situations. For instance, I think that in Siberia there are cases where there are three different sizes of aggregates during the course of the year.

LEACOCK: Yes, we will have to define patterns of attributes rather than merely note maximal and minimal group sizes.

GARDNER: We would assume that a large group would be established for a season, or for at least a reasonably prolonged period, and would be together not just for ceremonies but for other reasons as well.

HELM: Would we not then be concerned with the maximal aggregated unit, not just the maximal band unit that might not come together as an aggregation?

McFEAT: These points might prove interesting in terms of evolution, in that they relate to another point that has been made, namely that groups tend to enlarge. Also, if this were a tendency that we could assume to be true, which would influence other factors, then it might be an index to change.

McKENNAN: I think that one of the most interesting tangential observations that has come out of our discussion of size of groups was the fact that

under conditions of marked acculturation the viable group has become noticeably larger.

McCLELLAN: Regarding other categories under settlement pattern, would we not have to consider 'duration of maximal-minimal aggregations'?

GARDNER: That category would fit well under settlement pattern. It is really an assessment of different settlement patterns that occur at different times of the year. Perhaps this factor could be encompassed in our scheme.

HELM: These factors were built into Bicchieri's original definition. His "dispersed-cohesive" groups, for instance, are dispersed on the band level but do come together with more regularity than do the "dispersed" groups, which are dispersed most of the time and aggregate in a random fashion.

LEACOCK: This brings up the question of relative fluidity or stability of aggregates.

GARDNER: Regarding the turnover of population, if an Eskimo band has 60 to 70 per cent of the same people each year, and if the Birhor group has a higher percentage and the Shoshoni group a lower percentage, this may be relevant because the degree of stability in this regard affects the potential ties with other groups. If it is a small group, it must make contact with others in order for marriages to take place.

LEACOCK: We would have to consider the turnover or membership stability of the different units in a society.

DAMAS: One problem with this category is that some of our bands do not achieve aggregation. I understand this to be true in the case of the Déné.

HELM: They may have aggregated at fisheries, but this aggregation, if it did occur, would not last three to four months as in the case of the Central Eskimo sealing village. That is the critical difference—the Eskimo could stay together.

DAMAS: That situation among the Central Eskimo reflects the subsistence base. Only when superior resources were available could small Eskimo groups exist independently during the winter sealing season. I suppose we can expect periodic aggregations in most societies for social, if not for economic, reasons, but certainly in the case of the Central Eskimo the economic reasons were important.

McCLELLAN: The articulation would be different if people aggregated for social reasons than it would if they aggregated for economic reasons. There would also be variation. The group assembling for the potlatch might not promote the same sorts of aggregations as we have been speaking about in this Conference.

DAMAS: It is always very difficult to determine why people aggregate. On that basis, would it be feasible to try to separate societies in terms of motivations for aggregation?

HELM: I feel that perhaps different kinds of social directives become established in a society in which people stay together in large aggregates for

four or five months of each year, as opposed to a society with perhaps a similar number of people who form aggregates for periods of only a week or two. This may be a base on which certain sorts of social arrangements recur. Our problem would be to determine the manifest function of aggregates.

McCLELLAN: In classifying these matters, I think that we should separate duration from fluidity of aggregates.

LEACOCK: That being the case we should then add two more categories to our scheme: *duration of aggregates* and *aggregate membership stability*.

McFEAT: We should probably include something on 'aggregate differentiation,' for instance, Slobodin's various types of Peel River groups.

HELM: In other words, we should include not only the societal unit but also the non-societal units like the trapping party.

SLOBODIN: These would be 'task groups' in your terms.

HELM: Some of these task groups function almost like small societies, as, for instance, the Birhor bands. This has to do with the function of aggregates.

LEACOCK: We could probably use the heading *types of aggregates* to cover these problems.

In addition to the criteria we have named, there is also the question of the nature of ties with other aggregates.

HELM: There are two problems here: first, the relationships between minimal and maximal aggregates that comprise the band; secondly, those relationships between maximal aggregates which comprise the tribal level of association. These two categories then, *internal aggregate relationships* and *external aggregate relationships*, could be added to our list of criteria.

With these several criteria we have perhaps laid out an outline for assessing the dimensions of settlement pattern which will provide a base for an analysis of actual social relations. These criteria, under "settlement pattern," could be summarized as follows: (1) *minimal-maximal aggregates*; (2) *duration of aggregates*; (3) *aggregate membership stability*; (4) *types of aggregates*; (5) *internal aggregate relationships*; (6) *external aggregate relationships*.

COMMUNITY PATTERN

HELM: In shifting the discussion to community pattern, one impression we seem to derive from this Conference is that a basic question in community composition is the relative weighting of laterality versus lineality in a given society or between compared societies.

LEACOCK: But do these form a single continuum?

DAMAS: This bothers me as well. When we speak about lineality, are we talking about absence or presence of lineage, or something else?

HELM: In addition to absence or presence of actual lineages, there is also the question of 'feel' for lineality.

EGGAN: It might be better to combine both these factors under such a term as 'skewing.' Very few of our cases will show 100 per cent emphasis on male or female principle; there will be many intermediate cases to be considered before we can form them into a type. Perhaps 'degree of lineal skewing' might be a way of phrasing the problem. If skewing were not present, a balanced bilateral situation would exist. If there were skewing, it would tend to shift the lineal or lateral emphasis toward one side or another.

HELM: Perhaps we can encompass both the residence problem and the descent problem by using an over-arching category of 'unisexual skewing.'

McCLELLAN: I do not like the idea of combining these two things, because in societies with matrilineal reckoning, skewing toward males can be prominent. I think that if we do not keep skewing and lineality separate there is going to be confusion.

DAMAS: I agree. I can envision a society like the Central Eskimo in which there may be a patrilocal skewing of 60 to 70 per cent without the presence of lineages, and I can see another type of band with lineages but which has a much closer balance.

EGGAN: Residence has the quality of either shifting back and forth or in some cases being stable. One must really follow the life-cycle approach to get a clear picture of residence. We might consider 'stability of residence principle' or 'variations in residence.'

McFEAT: Part of the dimension of locality that we want to identify is presumably the marriage pattern.

GARDNER: We must consider that shift in residence is not merely due to marriage. Elderly parents might shift residence to live with married offspring. Such situations may not have much to do with marriage rules but might reflect residence rules. The term *residence* is more applicable in such cases.

EGGAN: There are cases of avunculocal residence which take place at an early age, 8 or 10. It is a premarital situation which has nothing to do with marriage. It is a device used in a matrilineal society for lining up a series of men for task groups, for living together. Inheritance, though it may be marginal in these groups, is more important in other places.

DAMAS: Do we account for differences in family organization, for instance the nuclear versus the extended family, under community patterns?

HELM: The definition of 'nuclear' versus extended family primarily relates to settlement pattern, since the extended family is so identified because it is co-resident; it is a spatial arrangement. The settlement pattern comprehends how people are located or spaced vis-a-vis one another; for example, the principles operating to keep brothers together or to recruit sons-in-law.

LEACOCK: It would seem that the two categories 'principles of descent' and 'principles of residence' would express what we are seeking here better than would 'lineality' or the other possibilities we have considered.

McFEAT: There should be another category included here, either 'status differentiation' or 'non-kin status differentiation.' Achievement status as opposed to ascriptive status.

SLOBODIN: This does not seem to be taken care of under aggregates or settlement pattern. There is the example of caste or social class in complex societies or non-localized sibs in the simpler societies. There are other types as well, pseudo-kinship or non-kinship relationships. Status relationships that are not exemplified physically in aggregations would not come under principles of *descent*.

EGGAN: This would seem to relate to the degree of centralization which is probably important in community pattern. This category might be called 'political centralization' or 'degree of centralization of authority,' as, for instance, the presence or absence of a band leader.

DAMAS: Perhaps what we are seeking is 'degree of authority,' that is, authority structure, or authoritarianism versus egalitarianism.

EGGAN: Authority could operate with descent or without, but I do not know whether it is implicit to our scheme or whether this is a useful continuum for this level of analysis of community pattern.

There are a number of situations where other features remain unchanged, but status differentiation and (or) authority increases or decreases. If we think of that as significant in terms of a patterned type of change which, if continued, results in another type of organization, it seems to be that it would occur if there were considerable centralization and that should be correlated with some other things. *Status differentiation* should take care of these things.

GARDNER: *Status differentiation* would then subsume such features as informal leaders, nodes, kindred system, husband-wife differences, and so on.

McFEAT: Another category that certainly must be considered is one which has been referred to a number of times in the papers read here. That is *pattern of distribution*. The economic component of social relations cannot be omitted from a scheme of classification.

As before, we could summarize the different categories of 'community pattern' as follows: (1) *descent*; (2) *residence*; (3) *status differentiation*; (4) *pattern of distribution*.

SUMMARY DISCUSSION II

LEACOCK: The *community pattern* dimension generally refers to conventional categories, such as descent and residence. *Exploitative pattern* is also largely confined to concepts that have had considerable treatment in anthropological literature. This Conference has, on the other hand, focused on the *settlement pattern* — features of association that are built to a great extent upon the exploitative pattern. One way to climax these discussions might be to take the categories for settlement patterns that we have developed, 'minimal-maximal aggregations,' 'aggregate membership stability,' 'types of aggregations,' and so on, and develop them further.

HELM: Bicchieri has given a tentative division of band societies that might well be a guide to our discussion. If we take three societies from which we have adequate data — each one representing one of Bicchieri's types — and work through the settlement pattern categories, it may clarify and help to organize much of what we have done.

LEACOCK: Referring to Bicchieri's scheme, we should perhaps start with the lower end of the scale, his 'dispersed type.' Our representative group for that type is the Shoshoni.

SHOSHONI SETTLEMENT PATTERN

EGGAN: Referring to the first category *minimal-maximal* units, I would say first that among the Central Shoshoni there are two levels of minimal groupings. There is the elementary family, which, though existing during part of the year as an independent unit, is not one that can survive indefinitely alone. The minimum winter aggregation ranges between 10 and 25 persons.

The maximum is difficult to frame because it varies in different years. The range is probably between 75 and 150. The group that aggregated in the autumn for the piñon harvest, or for rabbit drives, or for the less frequent antelope drives, would be such a group. At that time there were ceremonies, alliances, shamanistic performances, and other social and ceremonial events. This aggregation could occur twice a year, but most commonly it took place in the autumn. These data are based on memory statements from Steward's informants. I suppose there may have been cases where one family wintered by itself, if there were an abundance of piñon nuts.

Regarding *aggregate membership stability*, my guess is that a nucleus of siblings tended to remain together during the winter. A brother-in-law might join such a group one year and be somewhere else the next year. The winter groups were relatively unstable beyond the nucleus of two or three siblings. The summer groups would be relatively unstable as well, in that a different area might be exploited and different combinations of one to three families would join together. I would guess that three-quarters or two-thirds of the people of one group might come into the same aggregations during two consecutive winters.

Under *types of aggregates* there is the winter encampment which was a regular sort of aggregate; the autumn gathering was another recurrent type. Beyond those groupings there were conceptual units which were referred to as the Piñon Eaters, the Squirrel Eaters, and the Seed Eaters, etc. (These

names were used by outside groups to refer to one another.) It was a vague group within a region, a regional band. At times there were task groups (in Helm's terms) which were organized for certain activities, and there were also trading expeditions.

With regard to *internal aggregate relationships*, I think that Steward made a detailed survey in which he circulated throughout the area and obtained standard survey data but perhaps tended to underestimate the degree of local cohesion. Harris reports a considerably greater degree of internal cohesion for the White Knife Shoshoni than Steward gives for the same and adjacent regions. Rather than there being merely a network of boundaries based on physiographic features, there may have been, in addition, more internal integration centred around ceremonies and leaders and perhaps sources of raw materials. The pattern of brother-sister exchange in marriage which often in succeeding generations became cross-cousin marriage was a cohesive force built on kinship.

The final category, *external aggregate relationships*, was characterized by evanescence among the Shoshoni. Indeed, contact beyond the family was irregular. There was neither any warfare between groups until after the white men entered the region, nor even feuding. Perhaps most external contacts were based on kinship, common language, and common culture. There seems to have been little difficulty in travelling except during the winter when the people were immobile because of deep snows and the lack of proper snowshoes. There was linguistic uniformity throughout the area.

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DAMAS (to Helm): There appear to be examples here of the regional band and of task groups, as you have used the terms. Do you find anything in this description that might correspond to the local band?

HELM: I should think that the Shoshoni winter encampment corresponds to the local band as I conceive of it, but I am not sure. The duration of the seasonal assemblage, if reconstituted by at least a core of the same personnel from year to year, would lead me to consider that grouping a local band. By 'local,' I might add, I do not mean that the group necessarily stays in one spot, but rather it has a locus within a travelled region. It has a locus of identification even though it is not fixed in residence at that point.

LEACOCK (to Eggan): Was there resentment between groups when they merged to exploit a piñon grove, or was any sense of ownership involved that might cause conflict?

EGGAN: There was no purpose in owning a piñon grove because for several consecutive years there might not be any piñon nuts, and wherever there were nuts, there were more than the people who could get there needed and more than the animals and the rodents could gather. The Shoshoni would gather them in blankets and sacks and would dig up the stores of rodents. Information regarding these supplies was not withheld but was spread very widely. Steward thinks that it was this irregularity that made territory merely a designation of where people were located without there being an association with ownership or control of territory.

LEACOCK: Perhaps we should now consider the Eskimo whom Service has designated 'anomalous' along with the Shoshoni.

DAMAS: It should be noted that Bicchieri has separated these societies in his scheme and placed the Central Eskimo further along on the continuum. It might be more appropriate to consider an example from the 'dispersed-cohesive,' which is the second category.

HELM: The representative of that category is the !Kung.

!KUNG BUSHMAN SETTLEMENT PATTERN

MARSHALL: Regarding *minimal-maximal aggregates*, the minimal unit will still be the extended family, which would consist of the father and his married daughters and sometimes his married sons, as well, if their bride service had ended. It might comprise 10 to 20 persons who stay together as a solid unit. The maximal grouping would be the band itself, composed of several families. The bands had from 8 to 47 persons with an average of 25. Larger aggregations occur when certain bands happen to converge in an area. The bands live separately and remain discrete. They may be camped a mile or so apart, but they use the same water-hole. They do not form a corporate body, but they do visit with one another. At one place in the !Kung territory four bands live together at one water-hole. In another place two bands use a common water-hole. The numbers of people that comprise the four neighbouring bands are 57, 38, 28, and 26. Those are average numbers for those groups.

The *duration of aggregates* depends on the rains. If the rains are good, there can be relative freedom of movement for about five months, January through May. During the months of the dry season, people are forced to camp near a water-hole. When water can be found in the hollows of trees and shallow pans, the families that comprise the band may separate and go to different places to hunt and gather food. A strong, extended family, the most stable unit of the !Kung, might go alone, or be joined by others, as they wish. A small nuclear family does not stay alone. When the surface waters dry up, the families must return to the band's permanent water-hole, where they live close together again (their greatest aggregation). During the dry season, the distances to which people can go to hunt and gather are limited by the amount of water they can carry from the water-hole in their ostrich egg shell water containers.

Aggregate membership stability could be considered quite high, although it may not appear so. On the one hand, members of the groups often move from place to place visiting relatives and friends, yet even though they are away from their band area they remain members of their own bands. Furthermore, people have some choice and may change their membership from one band to another (marriage and bride service being the chief factors in this but not the only ones); however such changes would usually take place perhaps only once in a lifetime and rarely two or three times.

When considering *types of aggregates*, in addition to family and band aggregates, there are three activities in which the !Kung group themselves — gathering, hunting, and dancing. There are no restrictions upon these group-

ings. People who like to do these things together arrange informally to do them, whether they are related or not, or whether they are from the same or different bands. Hunting and gathering parties are small. The dances bring together a number of these parties. People also join each other as they wish for travelling to visit or for going to their borders to trade.

Regarding *internal aggregate relationships* within a !Kung band, the members are bound by consanguineous and affinal bonds. Everyone in a band has a major bond with someone—that of parent-offspring, sibling, or spouse, and, like links in a chain mail, the people are bound to each other and to the headman, who is the owner of the resources (in a symbolic sense only, for he does not own them as private property). Obligatory bride service is important and accounts for the residence of a considerable portion of the people. A man in his first marriage must give bride service to his wife's people for about ten years; for a second marriage, the period is not so long. He may bring his parents and siblings with him if they want to come, and they, in turn, may bring others linked to them by major bonds. These relatives have to choose between following a young man, while he gives bride service, or remaining in the band where they were; the relative abundance or scarcity of resources might well be the determining factor.

There are no corporate aggregations higher than the band. The bands are autonomous and have no political interconnections. *External aggregate relationships* are nevertheless maintained among bands within the region by intermarriage and the resulting network of kinship bands which are continually being reaffirmed. Relatives and friends not only visit, but are involved in a system of gift exchange. Although this exchange operates on an individual level, it serves to mesh the people into a mutually obligated body. Furthermore, they have what we call a name relationship. They extend kinship terms to people who have the same names as themselves and their kin. Since the names go down in families from generation to generation and since there has been much intermarriage, the same names are in use throughout the region and there is a kinship term for every name. This fictional projection of kinship gives an entity to the region in which intermarriage takes place but does not encompass !Kung speakers as a whole, for there are !Kung no more than a hundred miles away from the Nyae Nyae region whom the Nyae Nyae !Kung call by the same word they use for stranger. The region in which intermarriage with concomitant gift exchange and name relationship operate comprises about a thousand people.

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HELM: Is the minimal grouping, the extended family, much smaller than the band?

MARSHALL: !Kung bands usually were formed of two or three extended families. There might be separate nuclear families in the band as well. Several bands comprised one extended family which included three or four nuclear families.

HELM: Is there such a thing as ownership of a water-hole?

MARSHALL: Yes, at least as I interpret it. A band has rights to a water-hole or a water-hole belongs to the band in some way that can be considered ownership. Two or more bands may own or have rights to the same water-hole jointly. However, though the water-hole may be owned, water is not so strictly held for the use of the band as the vegetable foods are. In the permanent water-holes, few though there are, water keeps coming, whereas the plant foods are not available till the next year. Water would not be withheld from visitors or travellers.

LEACOCK: Do the vegetable resources ever fail for one band? Are these owned resources predictable or stable?

MARSHALL: If there were a drought affecting these resources, it would most likely cover the whole area, not just the territory of one band. The roots and nuts that are gathered may be scarce in drought years, but there are no stories of actual starvation. At least 80 per cent of the !Kung's daily food is vegetable and is gathered by the women daily.

HELM: Is there evidence of a trend toward a lineal emphasis among the !Kung?

MARSHALL: I do not find a corporate lineage, but there is a particular emphasis on males and the male side of the family. The father in the extended family is the leader. A father has the right to name all his children, and there is a predominance of naming for the father's relatives rather than the mother's. Inheritance of headmanship goes from father to oldest son. There is no formal provision in their social structure for this status to pass through the women. More men are actually in uxorilocal than in patrilocal residence because of the long period of obligatory bride service. They may choose to stay in uxorilocal residence for the rest of their lives, but this is not the society's requirement and must be separated from the male orientation in organization.

LEACOCK: Earlier you mentioned the extended family as comprising a father and his married daughters and possibly his married sons. How does the system of marriage work? Do they marry out of the lineage and not out of the band?

MARSHALL: The band as such is not the measure of endogamy or exogamy. Consanguinity and the name relationship regulate marriage. Consanguineous relatives who must not marry include parents, offspring, siblings, nephews and nieces, father's and mother's siblings, and all first cousins. Theoretically, second cousins should not marry, but we believe the !Kung are not so strict about the latter as they are about first cousins. It would seem that they could not be. We did not find spouses with a common grandparent, but we have little data on great grandparents.

HELM: In Marshall's material there are certain important factors not generally encountered in the other groups that have been considered in this Conference. (1) The resource is there, but it has to be husbanded; (2) there is a strong sense of ownership of resources; (3) there is a strong sense of regularization of transmission of headmanship. This contrasts, for instance,

with the Shoshoni where resources may fluctuate in supply, but they are not husbanded when they are present; there is also a lack of regulated authority and ownership.

DAMAS: So far, we have considered two groups that represent categories (1) and (2) in Bicchieri's scheme, that is, the lower part of his continuum. For a third grouping it might be well to use a society that represents something closer to the upper end. While we are not here represented by a society that would fall into the agglomerated type, we do have the Central Eskimo which Bicchieri has classed as cohesive-agglomerative of type (4).

CENTRAL ESKIMO SETTLEMENT PATTERN

DAMAS: Regarding *minimal-maximal aggregates*, the minimal groupings may at times be the nuclear family. This would, however, be only in the Copper Eskimo area and would last for only a few days. The typical minimal unit would be about twenty persons. In the Iglulik and Netsilik areas it would commonly comprise an extended family. I have given a range of five to fifty for the hunting group, but both extremes are short-lived, as noted above. The maximal aggregate occurred at the winter sealing village. Average for the maximal unit would be about one hundred with a range from about fifty to one hundred and fifty in the aboriginal period.

Duration of aggregates would vary. The winter sealing village could be at its maximal size for two months or for as many as five. In addition, as I have noted in my paper, the sealing village might be split into two components which would be within easy communicating range of one another. Movement of personnel back and forth between the components would be frequent. Duration of the minimal aggregates would be more difficult to compute. If the account of Jenness depicts a typical situation, the Copper Eskimo hunting groups fluctuated in size and composition every few days. Accordingly, the duration of the minimal units was very fleeting. In the Netsilik area the minimal hunting groups might have had a duration of six or seven months but would occasionally merge with larger units at fish runs or at caribou drives. In the Iglulik Eskimo region it appears, as noted in my paper, that the basically extended family groupings divided for a period of a month or two into caribou hunters and sea mammal hunters. The whole problem of the duration of minimal aggregates is a difficult one to appraise because in most cases the minimal units merged with other units or were joined by peripheral members at some points during the more nomadic phases of the year's cycle.

Questions regarding duration relate closely to the next category, *aggregate membership stability*. I have noted in my paper that about 60 to 70 per cent of the members of the maximal winter aggregate of one year would form the core of the grouping the following year. In the case of the minimal units there is a variability in stability that parallels that of 'duration of aggregates.' I have reported that the Copper Eskimo hunting groups of the summer period may at times have been comprised of only one nuclear family. More often, these nuclear families combined for varying periods. These combinations appear to have occurred in a more or less random fashion, though kinship ties were probably often involved. Membership stability in these combined

groups might then be considered as being unstable. However, in the Netsilik and Iglulik regions, membership appears to have been much more stable for the basic hunting group of perhaps fifteen or twenty members. This core group, usually a close-knit extended family, at times took on a periphery of more loosely attached members. An examination of census data in these two regions for the periods of the year when the groups were most dispersed leads me to suspect that in a fairly typical extended family grouping, three or four of the constituent nuclear families remained together during most of a six- to eight-month period. Perhaps the same percentage of the constituent nuclear families made up the grouping from year to year. This would indicate an index of membership stability comparable with that of the winter assemblage of the winter sealing villages. This index does not, however, take into account the fluctuations in the total typical minimal unit which, as I have indicated above, often comprised more peripherally attached members. I would expect a lower degree of stability when these marginal members are included.

With respect to *types of aggregates*, I have designated as the band those people who aggregate at the winter sealing village. This is the regional band of Helm. The summer season hunting groups have been designated as hunting groups. They may have some correspondence with Helm's local band except that usually its membership fluctuated during the season of its existence. Perhaps her task group corresponds more closely to this sort of grouping. Hunting parties, as, for instance, polar bear hunts, comprised only of men, would be a clearer example of her task group.

In my paper I characterized the Iglulik and Netsilik bands as clusters of extended families and the Copper Eskimo bands as being fundamentally clusters of nuclear families.

Larger groupings appear to have occurred from time to time at least in the Copper Eskimo area. These groups, which were based on trading and ceremonial activity, do not appear to have had appreciable life spans, divorced as they were from an apparent lack of economic viability by their large size.

Internal aggregate relationships are chiefly achieved through kinship and partnerships. I have noted in my paper that the relative strength of these two systems varied among the three regions.

External aggregate relationships, that is, interband contacts, came about mainly through visiting. There is considerable movement among the personnel of the bands of each of the three major regions. The marriage universe is formed by a unit of five or six bands, which have been designated as the Copper Eskimo, Iglulik Eskimo, and Netsilik Eskimo tribes. In the beginning, a man often found a wife in one of the other bands of the tribal region or in his own band. Usually a period was spent in bride service. Among the Copper Eskimo this service took place before the couple was considered espoused, that is, while the young man waited for the bride to reach maturity. Afterwards the man periodically revisited affines. Perhaps one year out of three or four was spent in the band of the wife's parents if they were not co-resident. This reaffirmation of ties seems to have been a feature that continually reinforced contacts. Entire extended families also moved about in this way. They might spend a year or two in another band area, though usually there would be kin ties with members of the other band as well. Some

such movement can be related to the desire to exploit a new hunting area. In other cases the range of movement of a summer band might take persons outside the range of their home band's normal hunting area. They would then build up caches from the summer hunt, and when the winter aggregations formed, if they happened to be nearer the locus of the winter village site of another band, they might join it for a period, perhaps until the caches were exhausted or until the return of daylight made travel more feasible. Then they might join the original band for the duration of the sealing season.

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HELM: When you describe the Copper Eskimo bands as "clusters of nuclear families," do you mean that there are no primary ties between them?

DAMAS: No, it should be noted that the Copper Eskimo band charts look little different from those from the other two regions. The bands are held together by primary ties as are the nuclear families. On the other hand, the Copper Eskimo do not really have a highly developed organizational unit of the extended family, as is found in the other two areas. The leadership of one person or a distributive network that is automatically extended within the extended family is not present.

LEACOCK: Do the family heads within the extended family tend to be siblings?

DAMAS: The bond is stronger between father and son than between brothers. If the father is still living, the group is usually made up of the father, perhaps two sons, and a son-in-law. That would be a fairly typical group in the Iglulik or Netsilik areas. When the father dies, the bond begins to weaken.

EGGAN: That situation suggests lineality, in that the lineal bond is stronger than the horizontal bond.

DAMAS: There is indeed a tendency in that direction, but of course only living persons are involved. In a true lineage, would we not have the association with a deceased ancestor? If the lineage concept were present, would there not also be a greater solidarity among the surviving siblings?

EGGAN: I am not saying that there is a lineage concept present, but the situation that you describe seems to be the beginning of a lineage tendency. This situation might be related to the possibility that there is much to learn in the Eskimo society that gives great importance to the role of the father in teaching.

DAMAS: It is important to note that the splitting or hiving of sibling groups occurs at a wider level than the band. That is, after the splitting has occurred, brothers will often be found in adjacent bands.

HELM: If the clusters of nuclear families are related but no extended family organization is present, in what way is integration accomplished within the band?

DAMAS: Spouse exchange or partnerships of other kinds helped to structure the Copper Eskimo groupings.

HELM: That is, non-kinship factors might be mobilized within already existing spatial alignments.

DAMAS: I have noted that there is intra-regional variation in the relative strength of kinship and non-kinship factors in local organization. I do not know whether this typology of settlement pattern allows for such variation or latitude in the classification. Perhaps we should seek a classification that would envelop this level of variation.

Can we at this point make some generalization on the basis of the three groups we have examined?

EGGAN: We can say that there are a number of similarities in these three societies but also a number of differences, some major and some minor. There is a greater degree of complexity of organization apparent as we move from the Shoshoni to the !Kung to the Eskimo. Stripped down to minimal data like this the results do not appear to be especially significant. If, however, we had time to expand the analysis to the other criteria of classification that have been proposed, a clearer picture of the variations and similarities would emerge. If we take into account the differences that appear from group A to B to C, a continual expansion of some aspects of settlement pattern will be found. It is then important to determine the correlates of these differences to develop their evolutionary significance. It is agreed, however, that all we can hope to accomplish at this Conference is to sketch some of the possibilities. It is because the earlier typologists have omitted this point by point analysis and have been too dogmatic in their statements that our discussions have been worthwhile. It is possible that we might arrive at a reversal of the conceptualizations of band organization that have been proposed.

SLOBODIN: Since we have criticized Steward and Service, I should like to pay tribute to them, especially to Julian Steward, for starting our search for generalizations about band society.

HELM: I am sure we all fully join in in that tribute.

POSTSCRIPT TO BANDS: ON TAXONOMY, PROCESSES, AND CAUSES

By JULIAN H. STEWARD

RÉSUMÉ

Les traits culturels qui servent de diagnostic à l'identification d'un type particulier de bande peuvent être expliqués par des processus similaires comme, par exemple, ceux qui sont cause de la fragmentation ou, au contraire, de l'agglomération des familles en petits groupes; des processus similaires peuvent cependant être aussi déclenchés par l'interaction de facteurs d'environnement et de culture très dissemblables. On ne peut donc pas rapporter immédiatement ces facteurs ou ces causes aux caractéristiques taxonomiques. Dans toutes les régions, néanmoins, les agglomérations sociales irréductibles, — les bandes dites primaires, c'est-à-dire réduites au minimum élémentaire pour subsister, — semblent comprendre de 20 à 30 personnes au moins, ce qui constitue le minimum viable. Je pense que ceci est vrai en ce qui concerne les sociétés du Grand Bassin. Il est courant de voir les bandes primaires établir des relations et se marier entre elles là où les agrégats ne dépassent pas le nombre de 300 à 500 personnes, c'est-à-dire dans les bandes telles que les concevaient beaucoup d'ethnographes dans le passé. Des combinaisons particulières de facteurs peuvent provoquer la constitution de groupes intermédiaires de dimension, de composition et de durée variées. La concentration de ressources abondantes peut permettre à de vastes agglomérations de s'établir d'une manière permanente. Enfin, on peut observer qu'à l'époque contemporaine, beaucoup de bandes ont subi des modifications importantes par suite de l'intrusion d'institutions européennes, comme le commerce des fourrures; il importe de tenir compte de ces cas en tant qu'exemples de types particuliers de modernisation.

Because I was unable to attend this Conference on Bands, our editor has kindly invited me to make a few comments, especially in reply to Slobodin's criticism of my initial paper, "Observations on Bands." I should also like, however, to add a few thoughts on the nature of bands and to offer a caution about underestimating European influence on contemporary pre-farming societies.

TAXONOMY AND CAUSALITY

Slobodin, in quoting from my article of 1949, a statement to the effect that a taxonomy of cultural types is necessary before causal factors can be deduced, touches a very fundamental point. In my article, which discusses similarities in the development of early civilizations, I did indeed attempt to infer causes directly from cultural manifestations, but subsequent research has shown that expanding irrigation, which I had assumed to be the principal cause, had been unimportant in the early development of some states. In "Cultural Taxonomy, Factors and Processes in the Evolution of Pre-Farming Societies," published in the Conference on "Man the Hunter," I have ex-

amined the methodological inadequacy of this statement, for it violates what I had implied in 1936 but never made explicit. I can offer only the gist of my argument here.

On the premise that like causes produce like effects, it appears reasonable to assume that societies that manifest similar cultural characteristics, that is, similar effects, resulted from similar causes. Empirically, however, this assumption is not always valid, for these conferences have reported many cases, such as the Dogrib, Bushmen, certain Algonkians, Great Basin Shoshoneans, Australians, and other hunters, fishers, and gatherers, wherein dissimilar cultural practices in subsistence and varied environmental factors have brought about very similar social structures. In these cases, explanatory analysis must link cause and effect through process. Processes may be considered causes, but they may also be traced to more ultimate causes. The nature of the minimum, 'local' or primary band of twenty to forty persons is generally similar in the cases mentioned, because the basic causal factors, both cultural and environmental, have initiated similar processes, such as seasonal group fragmentation, aggregation, and trends toward cooperation, marriage patterns, and group composition. I stress the importance of an empirical, case-by-case approach, because this is the only means of ascertaining whether the same or different factors have activated the processes in each case.

The processes mentioned are largely cultural ecological processes or adaptations, although these do not preclude cultural historical processes, such as diffusion. Because ecological processes are initiated by the use of cultural factors or practices within different environments, I attempted in "Cultural Taxonomy . . ." to systematize the cultural factors on the basis of such functional categories as means of obtaining, preparing, and preserving foods; transportational devices; clothing; and shelter; and such environmental factors as the kinds, distributions, seasonality, and abundance of foods, climates, sources of water, geographical barriers, and other relevant features.

In the long evolution of the hominids, the invention and diffusion of cultural devices as well as climatic changes have from time to time entailed new processes that modified or transformed social structures. One need only consider the importance of such cultural factors as fire, worked stone for tipping spears and for cutting, fishhooks and fish nets, water craft, sleds, toboggans, and skin clothing to recognize how each may generate a variety of adaptive processes, as they are employed in distinctive environments. These cultural factors are reducible to perhaps a score of major categories and an undetermined number of subcategories, but the processes they initiate in social change also depend upon environmental factors. Dugout canoes and techniques for exploiting the abundant marine and riverine resources of the Northwest Coast were fundamental to the processes of demographic growth and population nucleation, which in turn stimulated processes of complex social elaboration and differentiation. It is important to note, however, that the Northwest Coast was more comparable structurally to the Pueblo than to the interior tribes of Canada, which obviously precludes deduction of ultimate causes directly from social characteristics. In their subsistence techniques, the interior Canadians generally resembled the Northwest Coast people far more than they resembled any farming tribes, but their

limited resources and harsh environments inhibited demographic growth and nucleating processes. In fact, the marginal, pre-farming societies throughout the world tended to be so responsive to year-by-year and month-by-month changes in environmental factors that Helm's emphasis upon the fluctuating composition of these small groups over short periods assumes great importance.

The characteristics of these small marginal societies, therefore, represented tendencies rather than strongly fixed patterns. In order to remain viable, most of them had to accommodate in size, composition, and interaction with one another when the basic factors varied. It is for these reasons, I suggest, that it has proven so difficult to define 'bands' either as a generic category, as a series of subcategories, or as some kind of subdivision of larger social units. Cross-cultural comparisons are tending to deal with 'models,' which seem to me to be abstractions based on certain structural characteristics that are related to processes and in some cases to causal factors, rather than with cultural types if these types signify more substantive representations of social groups. I wish to avoid the semantics of models versus types, however, and observe merely that the greater depth of understanding of causes and processes presently available has thrown doubt on the diagnostic characteristics that were once ascribed importance in cross-cultural types. This trend is encouraging even while it is confusing, for it may lead to a more systematic endeavour to identify basic factors. This task is not forbidding if the critical factors are viewed functionally rather than stylistically or formally. My efforts in "Cultural Taxonomy, Factors . . ." are a preliminary step in this direction.

MINIMUM AND MAXIMUM SOCIAL AGGREGATES OR BANDS

The data presented in these conferences indicate that most hunting, fishing, and gathering societies consist of population aggregates that range from a minimum size, which is usually no less than twenty-five to thirty persons—but larger in certain cases—through greater numbers to a maximum that has variously been placed at 300 to 500 persons. The earlier ethnographies usually dealt with maximum bands, which were described according to traditional preconceptions including the assumption that bands were clearly defined, strongly integrated, bounded territorially, and controlled by powerful chiefs. The picture today is that the minimum aggregate is the most permanent and strongly integrated social unit and that cohesion in most cases decreases with size, the maximum band frequently being little more than a group with which its members somewhat vaguely identify. Large, strongly integrated bands do exist, but they cannot be assumed everywhere.

Explanatory analysis, therefore, most profitably begins with the minimum band, which I designate the primary subsistence band—also called 'local' band and 'task group'—because it is the basic unit that cooperates in the tasks necessary to physical survival. This primary band normally consists of no fewer than about twenty-five persons—or some five families—because, owing to its isolation during much of the year, approximately this number is necessary to ensure mutual aid in sickness, baby tending, and various cooperative activities including food sharing. Cases of one or two isolated families in aboriginal times are almost certainly abnormal, and such small aggregates

either demise or amalgamate with a large group. The size of the minimum band might theoretically be reduced in areas of extreme population density, where bands are readily accessible to one another, but such density usually entails larger aggregates and sometimes even permanent settlements. In recent cases of seasonal fragmentation into one or two nuclear families, the effects of firearms, commercial fur trapping, and other European influences are usually discernible.

The small primary band tends strongly to consist of persons who are consanguineally and (or) affinally related, for multiple bonds are thus created between families. This may be an avowed cultural preference, as among the Shoshoni, who specifically desire multiple marriages between the siblings of two families. Such family alliances may consist of sister exchange or also of marriage of several brothers to several sisters. All members of these primary bands, however, would become consanguineally related if they were wholly endogamous within four or five generations, even though they began with five or six entirely unrelated families, and band exogamy would be necessary. A strong marriage preference for sister exchange would, however, decrease the number of families with eligible marriage partners to one-third and require band exogamy sooner.

Where primary bands average some twenty-five persons, there will be twelve to twenty such bands within a maximum band of 300 to 500 persons. Marriages are presumably contracted with the most accessible band which is already linked through kinship, until consanguineal relations are established to the extent of requiring search farther afield. We have few precise data on the actual number of interband contacts or genealogies showing kinship linkages. Highly tentative estimates suggest that a given primary band may normally contact no more than one-half or two-thirds of the other bands in the course of a year, but this number—some 150 to 250 persons—should afford an ample number of marriageable persons. Moreover, in the absence of mythological or fictitious descent groups, consanguinity is rarely traced beyond the fourth or fifth generations, after which descendants of the same ancestors may marry. On the other hand, polygyny, or a succession of marriages by an individual, will extend consanguineal ties. I venture to guess, therefore, that marriage ties radiate outward from a given primary band to include others with decreasing frequency but rarely extend to all members of the maximum band. Each band, however, is linked to others, so that the kinship bonds within the maximum band will constitute a large, if diffuse, network.

This rather theoretical statement covers cases where there are no marriage proscriptions except consanguinity as reckoned through the first and possibly second cousin relationship. Such prohibitions, of course, are culturally proscribed, as are the basic incest tabus within the nuclear family; but if the society lacks access to eligible mates, it may have to redefine consanguinity or ignore proscriptions if it is to survive. Eggan has mentioned even brother-sister marriage among certain extremely small and isolated Lacandon groups.

Whether such traditional or diffused patterns as clans and moieties elsewhere may not seriously reduce the number of eligible marriage partners is an interesting problem. A moiety system would reduce the eligible marriage partners in the maximum band by one-half, which could impose serious

limitations. Among some Algonkians, who have strong patrilineal tendencies, and western Athapaskans, who have matrilineal clans and in some cases moieties, cross-cousin marriage is frequently reported. Although cross-cousin marriage may be explained in a number of ways, it would also serve to reduce the restrictions imposed by descent groups.

I suggest, then, that a society may, under certain circumstances, have to modify its historically derived conceptions of consanguinity if ecological processes make this a functional necessity. There are cases that show a strong tendency toward unilineal bands and local exogamy. Where the primary band is a patrilineage, its size is usually somewhat greater than the minimum of twenty-five, which can be attributed in many of the cases I reported in 1936 to myths that perpetuated a fiction of relationship where it is not genealogically traceable. I explain this by the nature and importance of hunting, which tends to be far more cooperative than seed collection. Most of my cases cannot now be reassessed, for relevant data are beyond recovery, but I pointed out previously that this is really no more than a tendency that may be modified by many factors.

Whether the primary band has unilineal tendencies, and precisely how its members are related to one another and to persons of other bands through kinship require fairly detailed knowledge of the basic causal factors. It is not enough to say that a large percentage of food is fish or game or that a society hunts and traps certain species. McKennan has described how earlier Central Alaskan Athapaskans hunted caribou with fences up to 30 miles long, the building of which required cooperation between members of a group of 35 to 65 persons. Rifle hunting of moose (moose have replaced caribou in many areas) is a factor that generates very different social processes. It would be highly informative to know exactly what kinds of group effort fishing entailed where different kinds of devices and species were involved, who cooperated in fishing, and what quantities could be stored. It would also be important to know whether different methods and kinds of cooperation were involved in hunting various kinds of large game, and how game was shared.

While the primary band seems to cooperate in most endeavours, joint participation of several bands is more restricted. In some cases, such bands appear to participate in social life only when local and seasonal abundance of food permits. There are cases, however, where the winter habitation, such as the Central Eskimo winter sealing village, consists of 60 to 100 persons. Except for such instances, the larger social aggregates seem generally to be loosely amalgamated, the network of kinship being a principal bond. The boundaries of the maximum bands tend to become vague unless marked by deserts, mountains, bodies of water, or other natural barriers, and it is by no means certain that these 'bands' have clear dialectical or political frontiers or that they even represent the limits of marriage alliances.

MODERNIZING FACTORS

Many of the studies in the conferences on Bands, Hunters, and the Great Basin reflect European influence to the extent that doubt inevitably arises whether the societies are to be considered truly aboriginal. In nearly all cases, ethnohistory would, if available, have enormous value. In some cases,

external influences seem such that the problem might be better phrased as one of modernization than as a comparative study of aboriginal societies.

Changes induced by Europeans may be of three kinds: first, modification of environmental resources; second, use of manufactured devices in exploiting environments; and third, social linkage with larger external national institutions. Since these societies are sensitive to changing factors, any of these alter social structure in important ways, even though the general culture appears predominantly aboriginal and the people still live off the land.

Lee describes the Bushmen as aboriginal even in their subsistence devices, but he also states that wild game has been reduced and that Bantu have entered part of Bushman territory with herds of cattle. It seems possible, therefore, that Schapera may have been correct forty years ago in reporting patrilineal bands, but that subsequent changes have altered this type of society. Game in many other parts of Africa has been diminished, except on reserves, and hunting in earlier times may have contributed more than the 20 per cent of subsistence now estimated.

In South America, from the Gran Chaco to Tierra del Fuego, livestock has virtually replaced wild game, especially the guanaco. North America has witnessed a more drastic reduction of bison and mountain sheep, whereas deer and antelope have today a distribution different from formerly. Northern Canada has not been stocked with domesticated species, but there are many areas where moose have replaced caribou.

While some societies continue to use aboriginal means for hunting, a few allusions to rifles, such as Slobodin's mention of their use by the Kutchin in caribou hunting, suggest that they are too common in interior Canada to evoke mention. In fact, I have the impression, although I cannot be sure, that the bow and native types of traps have everywhere been replaced by rifles and steel traps, except in a few cases such as the Eskimo, who find the native-type of harpoons most efficient for sealing. The kind of social cooperation necessary for rifle hunting is surely different from that which McKennan describes for earlier use of the long caribou fences. The steel axe also has expedited many chores of peoples who make many things of wood, for example, dugout canoes and log cabins used by the Carrier.

The linkage with external institutions may have even more profound effects than simple adoption of industrial age weapons and tools. The fur trade has long been the principal linkage of Canadian societies with the larger world, and Leacock reports that it began as early as 1534 among the Micmac and that trading posts were widely established in Canada by 1670. The dependency on the fur trade is difficult to assess, for the extent to which steel traps were employed, the time devoted to taking furs, and the degree of dependency on the trading posts for such essentials as food are rarely discussed. We note that Slobodin's trapping party carried purchased food and supplemented it with the results of rifle hunting. The fur trade entailed visits to trading posts, which were commonly at fishing sites, where primary bands might associate more frequently. Rogers mentions that certain large eastern Algonkian bands consist principally of groups that assemble at trading posts. Missions and schools, which frequently are located at the trading posts, had attained such importance that families of some areas endeavoured to make these permanent residences or headquarters so that children might attend school.

Although the effect of the fur trade is obviously only incipient in some areas, it has culminated in the complete individualization of land use in others. Many years ago, Frank Speck argued that the family trapping territory was aboriginal, and John M. Cooper used Speck's assumption to postulate a comparability of the family trapping territory and the Fuegian band hunting territory as an age-area historical reconstruction. Convinced that the trapping territory was a response to the fur trade, I argued this point with Speck in print and in person. Later, my own work among the Carrier of Stewart Lake in British Columbia, as well as reports by Jenness, left no doubt that these territories developed as a result of institutional linkages with the larger national economy.

A brief account of the Carrier may be in order since most of my material and genealogies are unpublished and my short articles have been overlooked. In prehistoric times, the Stewart Lake Carrier had simple hunting and fishing bands, the exact nature of which is not known. In late protohistoric times, trade of furs via the Tsimshian to the Coast created a new source of wealth, after which an attenuated system of matrilineal moieties, statuses, and potlatching spread among most Carrier. The Carrier modifications of the moieties may suggest interpretations of the matriclans and moieties mentioned by McKennan. At first, nobles claimed the fur resources of specific areas and took the name of one or the other of the Tsimshian moieties, so that several different territories and their nobles bore the same name. Eventually, a third, fourth, and fifth moiety name was introduced, so that the socioterritorial divisions became more like clans.

At the time of my field-work in 1940, however, inheritance of territorial resources by the sister's son had been abolished a generation earlier, when each noble divided his trapping areas among his own children. These territories had been registered with and were protected by the Provincial Government, and they even included territories that had been lost to Europeans. The Carrier then maintained their principal headquarters at the trading post, with smaller communities scattered elsewhere. They depended largely upon commodities obtained through trade and secondarily upon moose hunting with rifles.

I am not sure how far northward the family-owned trapping territory has spread, but I get the impression that most if not all Canadian tribes and Eskimo are involved in some trade. Where such territories occur, there is a dual economy and system of land rights. Families own trapping rights along sections of streams and rivers where they practise conservation. Any member of the maximum band may, however, hunt larger non-furbearing animals. I get the impression regarding the Athapaskans and the societies in other parts of the world that so-called 'band territories' are today, and perhaps always were, simply areas of habitual use, often separated from other territories by natural barriers, rather than areas to which exclusive rights were claimed and protected against other bands. After all, how often could members of the maximum band mobilize their entire manpower to repel intruders? Are there any such recorded cases? Or do hostilities have more the character of feuds between families? I also get the impression that people in the far north hunt wherever they find game in their seasonal treks but that the nature of large game hunting has been changed by use of firearms.

McKenna reports that the use of dog sleds has spread westward. Water transportation in much of Alaska has been vastly improved by use of outboard motors. Commercial fisheries have so dominated much of the Coast that tribes such as the Tsimshian and Tlingit, which I observed in 1940 on the lower Skeena River, had become completely proletarianized, living in company shacks and depending entirely on wage labour.

These comments are not intended to question the intrinsic importance of the accounts of contemporary societies, whether these societies have been only slightly touched by European influences or drastically transformed. It is important to glean what is aboriginal while we can, and there are rich potentials in ethnohistory as many of these papers show. But the time has come when most societies throughout the world are necessarily studied in terms of the factors and processes of modernization. Pre-farming societies have been largely neglected in such studies in favour of the large farming and pastoral peoples, who constitute the basic populations of modern nations.

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DATE	ISSUED TO
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CONSTANT		MINIMAL LEVEL OF SOCIO-CULTURAL INTEGRATION: FORAGING		
CHOSEN VARIABLE	DESERT	SCRUB FOREST	TROPICAL FOREST	
SAMPLE GROUPS	SOUTHERN KALAHARI DESERT BUSHMEN	CENTRAL EAST AFRICA HADZAPI	NET-HUNTING BAMBUTI OF ITURI FOREST	
A. SETTING				
1. HISTORICAL RECORD	Oldest living inhabitants of Southern Africa Probable Northeastern provenience	Little record Probable prehistoric connection with southern Bushmen	Oldest living inhabitants of Congo basin Probable local origin — special relationship with Negroes	
2. PHYSICAL TYPE	Height: 160 cm. Body: slender; extended belly; steatopygia; penis-erectus; hottentot-apron. Colour: yellowish-brown. Hair: body, scarce; head, peppercorn. Head: mesocephalic. Face: broad-flat; prominent cheekbone; low-bulging forehead; broad-low nose; short-broad ears; thin lips; dark slanted-looking eyes	Height: 160 cm. Body: slender; extended belly; some steatopygia. Colour: light-to-dark brown, black. Hair: body, scarce; head, peppercorn. Head: mesocephalic. Face: broad-flat; some cheekbone prominence; both thin and averted lips; flat-low nose; broad ears; high variation due to hydridization	Height: 140 cm. Body: slender; long upper limbs; infantilism. Colour: dark-brown reddish hues. Hair: body, lanugo; head, tightly curled. Head: meso-brachycephalic. Face: medium-broad; high-bulging forehead; broad-flat nose; medium-thick non-everted lips; brown eyes	
3. LANGUAGE (1) Family	Click	Click	Niger - Congo	
(2) Sub-Family	Khoisan	Hadza	Bambuti	
4. HABITAT				
(1) PHYSIOGRAPHY	3,000-ft plateau, arid, sandy - rocky	Mountainous ridge, rocky, loamy - clay soil	Tropical forests; humid - rainy; minimal sun penetration	
(2) CLIMATE: General	Hot, dry	High variation, cold-to-torrid, moist-to-windswept	Hot, humid	
Temperature	Mean, annual below 60°F	Marked seasonal and daily variations: 50°F - 115°F	Mean annual ±80°F	
Seasons	Hot summer with rain; winter with possible frost, drought	February - May, rain, hot; June - January, dry, windy	Little seasonal change	
(3) WATER: General	Very scarce	Limited	Abundant	
Rainfall	Less than 15 inches per annum	±25 inches per annum	±100 inches per annum	
Ground	No permanent running; moist riverbeds; some water pans; rare permanent waterholes	Limited but normally available in natural storages: wet-sandy rivers, rock and tree hollows	Permanent, abundant, ubiquitous, streams	
(4) FLORA: Kind	Thorn-bush; xerophits; berries; cucurbitaceae; roots	Thorn-bush; small, thick forests; berries; roots; vital Baobab	Fruit; berries; roots; nuts; mushrooms; tendershoots	
Locality	Varied distribution with seasonal variation	Patchy distribution; seasonal variation	Ubiquitous	
Quantity	Scarce to sufficient with seasonal variation	Limited, sparse; seasonal variation	Abundant	
(5) FAUNA Kind	Antelopes; zebras; giraffes; ostriches; busterds; partridges; grouse; reptiles; insects (honey-bee)	Antelopes; zebras; giraffes; buffalos; elephants; ostriches; partridges guinea fowls; reptiles; insects (honey-bee, tse-tse)	Antelopes; wild boar; buffalos; elephants; anteaters; monkeys; birds; fowls; reptiles; insects (honey-bee)	
Locality	Concentrated and dispersed, according to season near food	Dispersed, proximity of water; some seasonal change	Dispersed in thick forest	
Quantity	Scarce to sufficient with seasonal variation	Sufficient but dispersed; some seasonal change	Abundant	

B. MAN TO HABITAT

SUBSISTENCE DEVICES: Location	Migratory; grass shelters; move with food, water supply	Migratory; grass shelters; move with water, game supply	Cyclic; leaf-thatched shelters; relocated in campsite convenient for clearing and clean water
Activities	Strict sexual labour division	Strict sexual labour division	Minimal sexual labour division
Men	Hunting and related activities	Hunting and related activities	Hunt, collect, camp building, and related activities
Women	Food - water - firewood collection; camp building; etc.	Food - water - firewood collection; camp building; etc.	Food - water - firewood collection; camp building; participate in hunt; etc.
Techniques: Men	Bow - arrow (poison); tracking; pursuit; disguise; spear; club; fire	Bow - arrow (poison); stalking; platform shooting; trap; small ax; fire	Nkusa-vine nets; game drives; bow - arrow (poison); spear
Women	Dibble-stick; vegetable and leather bags; egg-shells	Dibble-stick; vegetable and leather bags; egg shells	Dibble-stick; vegetable basket; branches for drives

C. MAN TO MAN

1. FAMILY (Basic Social Unit)			
Nature	Semi-patriarchal; nuclear; basic permanent unit; rare polygyny	Strongly patriarchal; nuclear; basic, permanent, semi-independent unit; polygyny	Minimally patriarchal; nuclear; minimal permanent unit; band dependent; polygyny rare
Marriage	Incest taboo; band exogamy; occasional levirate	Incest taboo; band exogamy; occasional levirate	Incest taboo; band exogamy; levirate; "sister exchange"
Prerequisites: Male	Able provider	Able provider	Able provider
Female	Passed menarche and subsequent instruction	Passed menarche and subsequent instructions	Passed menarche and subsequent instructions
Residence	Prevalent virilocality; some bride-service	Mostly patrilocal	Prevalent virilocality but common exceptions
Procreation	Limited male dominance; mostly spouses egalitarian; divorce rare, children to father; babies generally welcome but infanticide occurs	Male dominance; fairly egalitarian spouses relation; divorce rare, children to father; babies welcome but infant death not grieved	Spouses egalitarianism
			Divorce rare, children to father; babies very welcome
2. EDUCATION (Continuity of Culture)			
Early	Permissiveness; adult-imitation play; observer-participant	Permissiveness; adult-imitation play; observer - participant	Permissiveness; adult-imitation play; observer - participant
Puberty	Some formal ritual	Little formal ritual	Participation in Negro formal ritual and some indigenous semi-formal ritual
Boy	Minimal formal instruction	Minimal formal instruction	Participation in Negro-village puberty rites
Girl	Formal instruction at menarche	Formal instruction at menarche	Participation in Negro-village puberty rites; indigenous menarche instructions
3. GROUPS (Commitment to Social Life)			
Minimal: Family	Limited semi-independence during drought; 4 - 5 members	Independently efficient economic unit; 4 - 5 members	Minimal independent unit; 4 - 5 members

COMPARATIVE GENERALIZATION

DERIVED REGULARITIES

STEWARD'S POSTULATIONS

UNIVERSAL CULTURE TYPE EXPLOITATIVE STATEMENT

At the foraging level of cultural complexity, the cultural core constitutes most of the total behavioral pattern, and most activities are devoted to the exploitation of natural resources by means of a very simple technology — the critical food source is, often, other than game.

NON-HABITAT SETTING

Significant items to be used to interpret the nature and value of historico-cultural factors.

PHYSICAL ENVIRONMENT

At the level of cultural complexity being considered, variation in the constituent elements of the habitat, such as physiography, climate, water, flora and fauna produce significant variations in the culture-core. All of these variations (rather than variation caused by fauna alone) are essential attributes of each individual society.

COVARIATIONAL CLASSES

1

In groups where the culture core constitutes most of the total behavioral pattern, some cultural regularities are observed, such as . . .

2

As the exploitative technique, in response to the specific habitat, varies from individual to cooperative . . .

3

As the habitat varies from restrictive to permissive in providing the means of livelihood . . .

- Uniformity in distribution of vital natural resources and thickness of vegetation is accompanied by cyclic reuse of campsite.
- Subsistence based on intra- and inter-familial cooperative action is accompanied by minimization of sexual division of labour.

- Basic bow - spear - club and dibble-basket technology. General knowledge of poison.
- Hunting - gathering techniques, use of fire, storage of water vary with habitat.

- Patriarchalism most marked where sexual division of labour is strict. Polygyny permissible but rare.
- Incest taboo, band exogamy general. "Sister exchange" where band size inflexible.
- Hunting ability.
- At menarche specific instruction on wife, mother role.
- Prevalent virilocality correlated with strict sexual division of labour.
- Spouses egalitarianism, maximized where sexual division of labour minimized.

- Divorce rare. Children desired but mortality rates (natural and man-made) vary with permissiveness of habitat.

- Early, permissive; most learning by doing in actual situation and in ritual and recreational context.
- Little ritual formalization. External cultural contact explains presence of puberty rites.
- Informal instruction spread over time.
- Formal instruction at menarche.

- Autonomy of family varies with variation in exploitative devices; also temporary affect of seasonal

. . . Male dominance results in basic viri-orientation, a situation which is significantly modified by variation in the habitat.

. . . Menarche is stressed by formal instructions while boys' puberty is not. More elaborate puberty rites are derived from cultural diffusion.

. . . Band, of variable composition.

. . . Sexual division of labour is de-emphasized.

. . . Viri-orientation in terms of patrilocality, patrilineality, and patriarchy is de-emphasized.

. . . Opportunistic migration replaced by cyclic reuse of campsites.

. . . The attitude toward the very young and the very old becomes more accommodating.

SETTING

NON-HABITAT

Important in explaining nature of secondary features which are determined, to a great extent, by purely cultural - historical factors.

HABITAT

Limited and scattered resources. Variations, other than of fauna, are mentioned, but as secondary considerations.

Fauna

Small, non-migratory bands of game constitute the principal food resource.

MAN TO HABITAT

Activities:

Generally: Sociological effect of hunting scattered game individually or collectively is the same everywhere.

All the same, not on account of total environmental similarity but on account of same nature of game. Differences not enough to change cultural adaptation.

Techniques:

Man: Hunter.
Woman: Gatherer.
Over-all similarity, only difference in detail.
Bow - spear - club.

MAN TO MAN

Patrilocality:

Local exogamy through fact or fiction of patrilineal relation. Caused by male dominance, greater economic importance of hunting-male.

EDUCATION

GROUPS

Family:
Band:

Patrilineal as consequence of dispersed small game bands and bow-spear.

C. MAN TO MAN

1. FAMILY (Basic Social Unit)

Nature	
Marriage	
Prerequisites: Male	
Female	
Residence	
Relations	

Semi-patriarchal; nuclear; basic permanent unit; rare polygyny
 Incest taboo; band exogamy; occasional levirate
 Able provider
 Passed menarche and subsequent instruction
 Prevalent virilocality; some bride-service
 Limited male dominance; mostly spouses egalitarian; divorce rare,
 children to father, babies generally welcome but infanticide occurs

2. EDUCATION (Continuity of Culture)

Early	
Puberty	
Boy	
Girl	

Permissiveness; adult-imitation play; observer-participant
 Some formal ritual
 Minimal formal instruction
 Formal instruction at menarche

3. GROUPS (Commitment to Social Life)

Minimal: Family

Limited semi-independence during drought; 4 - 5 members

Band

Viri-oriented group; ± 12 families; ± 50 members
 Basic kin-association altered to fit subsistence and personality preferences
 Politico-economic autonomy
 Territorial identification
 Linguistic identification
 Politically and economically insignificant

Maximal: Tribe

4. STATUS AND ROLE (Relationship Among People)

Status: Degree of Differentiation

Criteria: Sex	
Age	
Procreation	
Subsistence	
Personality	
Magico-Ritual	
Inheritance	

Small
 Little male dominance
 No longevity status
 Marriage - procreation gives full participant status
 Bio-subsistential performance important
 Rhetoric - wit and dispute setting ability valued
 Some ad hoc status
 Some present and related to scarce resources
 Family-to-band association; daily activities; socialization
 Leadership in location; subsistence activities; participation in band decisions
 Location - subsistence decisions; out-group relations; social control

Role: Familial
 Family-Band
 Band

AUTHORITY (Power to Regulate Behaviour)

General: Nature: Family	
Band	
Focus: Family	
Band	

Some independent decisions; generally dependent on larger unit
 Migration; hunting - gathering leadership; social control
 Preponderantly pater-familiae
 Mature, respected hunter; inheritance; band-consensus dependent

Law: Crimes
 Sanctions

Theft; adultery; homicide
 Ridicule, shame; ostracism

Agent

Band consensus; sanctioned self-help

N TO SUPERNATURAL

-lief

Mythical creator-god (mostly "moon")
 After-life associated with fear of death, belief in witch spirit

activities

Minimal ritual formalization; expression of anxiety; social-order oriented
 "Creator" prayed to in need, cursed in tragedy

nt

Some curing
 Ad hoc performers; both sexes; good and evil

NON-UTILITARIAN EXPRESSION

Dances, songs, games; simple instruments; some decorative art
 Animal mimicry; stress on desert fauna
 Significant function in enculturation and social-order maintenance

Strongly patriarchal; nuclear; basic, permanent, semi-independent unit; polygyny
 Incest taboo; band exogamy; occasional levirate
 Able provider
 Passed menarche and subsequent instructions
 Mostly patrilocal
 Male dominance; fairly egalitarian spouses relation; divorce rare,
 children to father; babies welcome but infant death not grieved

Permissiveness; adult-imitation play; observer - participant
 Little formal ritual

Minimal formal instruction

Formal instruction at menarche

Independently efficient economic unit; 4 - 5 members

Patrilineal kin group; ± 6 families; ± 25 members

Patrilineal kin-association

Politico-economic autonomy

Territorial identification

Some linguistic out-group contrast identification

Politically and economically insignificant

Small

Male dominance

Some longevity status

Marriage - procreation gives full participant status

Bio-subsistential performance important

Rhetoric - wit, compensation legislation ability valued

Very little ad hoc status

Sporadic

Family-to-band association; daily activities; socialization

Leadership in location; subsistence activities; participation in band decisions

Location - subsistence decisions; out-group relations; social control

Semi-independent from larger unit

Migration; hunting - gathering leadership; social control

Pater-familiae

Active patriarch with band-consensus dependence

Theft; adultery

Ridicule, shame; compensation

Patriarch; band consensus; sanctioned self-help

Mythical creator-god (mostly "sun")

After-life associated with neutral feelings

Minimal ritual formalization; social-order oriented

Respectful, non-interference attitude

Some curing

Very rare ad hoc performers

Dances, songs, games; simple instruments; some decorative art

Animal mimicry; stress on scrub forest fauna

Significant enculturation and social-order maintenance functions

Minimally patriarchal; nuclear; minimal permanent unit; band dependent; polygyny rare
 Incest taboo; band exogamy; levirate; "sister exchange"
 Able provider
 Passed menarche and subsequent instructions
 Prevalent virilocality but common exceptions
 Spouses egalitarianism
 Divorce rare, children to father; babies very welcome

Permissiveness; adult-imitation play; observer - participant
 Participation in Negro formal ritual and some indigenous semi-formal ritual
 Participation in Negro-village puberty rites
 Participation in Negro-village puberty rites; indigenous menarche instructions

Minimal independence (honey season); 5 - 6 members

Viri-oriented; ± 12 families; ± 65 members

Kin-association markedly altered to fit subsistence and personality preferences

Politico-economic autonomy

Territorial identification

Contrast-to-Negro-villager identification

Politically and economically insignificant

Minimal

Minimal male dominance

Longevity status generally granted

Marriage - procreation gives full participant status

Bio-subsistential performance important

Rhetoric - wit, conciliatory ability valued

Practically no status

None

Family-to-band association; daily activities; socialization

Leadership in location; subsistence activities; participation in band decision

Location - subsistence decisions; out-group relations; social control

Very few independent decisions; dependence on larger unit

Movement; hunting - gathering leadership; social control

Spouses share

Mature, respected hunter; band-consensus dependent

Uncooperativeness; theft; adultery

Ridicule, shame; exile

Band consensus sanctioned self-help

Mythical creator-goddess (forest)

After-life associated with benign feelings

Minimal ritual formalization; exalt forest; social-order oriented

"Communion" with forest spirit

Little curing

Magico-religious performers practically absent

Dances, songs, games; simple instruments; some decorative art

Animal mimicry; stress man - forest relation

Significant enculturation function, social order, and unity maintenance

TO UNIVERSE (VALUES)

ATE PREDISPOSITION

N'S RELATION TO NATURE

E DIMENSION

SONALITY

QUALITY OF RELATIONSHIP

Highly mixed

Man in nature

Present-centred

Being-in-becoming

Individualistic - collateral

Mixed

Man in nature

Present-centred

Being-in-becoming

Individualistic

Good

Man in nature

Present-centred

Being-in-becoming

Collateral

- Patriarchalism most marked where sexual division of labour is strict. Polygyny permissible but rare.
- Incest taboo, band exogamy general. "Sister exchange" where band size inflexible.
- Hunting ability.
- At menarche specific instruction on wife, mother role.
- Prevalent virilocality correlated with strict sexual division of labour.
- Spouses egalitarianism, maximized where sexual division of labour minimized.
- Divorce rare. Children desired but mortality rates (natural and man-made) vary with permissiveness of habitat.

- Early, permissive; most learning by doing in actual situation and in ritual and recreational context.
- Little ritual formalization. External cultural contact explains presence of puberty rites.
- Informal instruction spread over time.
- Formal instruction at menarche.

- Autonomy of family varies with variation in exploitative devices; also temporary affect of seasonal variations.
- Viri-orientation maximized where sexual division of labour minimized and where association is a social rather than an economic advantage.
- Politico-economic autonomy.
- Identification with specific territory (rather than ownership).
- Linguistic referent and/or contrast with other ethnic groups.
- Politically and economically insignificant.

- Small; decreases inversely with the amount of intra- and inter-familial labour cooperation.
- Male status increases as the rigidity of sexual division of labour increases.
- Negative correlation between harshness of habitat and status of old people.
- Validates adult status.
- Ability as provider important (male-dominance validator).
- Conciliatory ability valued; cultural value of humour correlates with harshness of habitat.
- Extemporaneous value.
- Hereditary transmission of property right correlates with scarcity of vital commodities.
- Inter-familial association; daily activities; socialization.
- Decisions on location and subsistence activities; participation in band decisions.
- Location - subsistence decisions; out-group relations; social control.

- Autonomy in family decisions correlates with degree of family independence.
- Location, subsistence, social-order decisions.
- Pater-familie more absolute where rigid sexual division of labour reinforces viri-orientation.
- Allocation of leadership varies with habitat, is always dependent on band consensus; inheritance correlated with scarce resources.
- Theft; adultery; uncooperativeness crime in societies stressing cooperative action.
- Ridicule and shame; ostracism effective in cooperative-action groups, compensation in independent-action groups.
- Band consensus; band consensus sanctioned self-help; patriarch where compensation is practiced.

- Mythical creator-god usually a natural element significant in the particular habitat.
- Fear of after-life correlated to harshness of habitat (i.e., the belief in witch spirits).
- Minimal ritual formalization but closely connected with social control.
- Amiability of man - creator relationship varies inversely with harshness of habitat.
- Little extemporaneous magic and curing, increases in harsher habitat.
- Magico-religious practitioner has only *ad hoc* function as part-time specialist with least status in societies with minimal fear of supernatural.

- Dances, songs, games; simple instruments; decorative art.
- Recreational activities then based on ecological referent.
- Significance of recreational activities in enculturation and maintenance of social order.

- From good to highly mixed according to harshness of habitat.
- Man in nature.
- Present-centred.
- Being-in-becoming, intermediate between passiveness and active striving.
- Individualism inversely related to adoption of cooperative action.

... Male dominance results in basic viri-orientation, a situation which is significantly modified by variation in the habitat.

... Menarche is stressed by formal instructions while boys' puberty is not. More elaborate puberty rites are derived from cultural diffusion.

... Band, of variable composition, is the maximal autonomous unit and is "identified" with a territory. The territory, as a complete unit of exploitation, is more permanent than the membership of the multifamily band which exploits it.

... Decisions are preponderantly subsistence decisions.

... Social order maintenance rests very heavily on informal controls provided by ritual and recreational activities.

... A mythical creator-god is present. This god is the anthropomorphized aspect of an environmentally significant natural element.

... The value orientation derived from man's relation to nature, to time, and to action are constant, not due to the nature of the particular habitat but to the common level of socio-cultural integration rooted in the lack of advanced technology.

... Viri-orientation in terms of patrilocality, patrilineality, and patriarchy is de-emphasized.

... Band size becomes less flexible.

... Focus of consensual decision is transferred from the family to the band.
... More stress is placed on crimes:
a. which threaten small group cohesion, such as breach of incest taboo, adultery, and theft;
b. which threaten livelihood, such as uncooperativeness in subsistence tasks.
... Sanctions stressing compensation are replaced by sanctions stressing socio-economic isolation.

... Patri-jural authority is replaced by consensual authority informally held by mature individuals exhibiting conciliatory ability.

... Nature and stress of ritual varied from isolative to agglomerative.

... The modality of relationship varies from individualistic to collateral.

... The attitude toward the very young and the very old becomes more accommodating.

... Informal authority focuses on achieved status rather than on status ascribed through hereditary transmission of scarce commodities.

... The relationship to the supernatural varies from fearful to amiable and the status of magico-religious practitioners declines.

... Innate predisposition varies from highly mixed to good.

MAN TO MAN

Patrilocality:

Local exogamy through fact or fiction of patrilineal relation.
Caused by male dominance, greater economic importance of hunting-male.

EDUCATION

GROUPS

Family:
Band:

Patrilineal as consequence of dispersed small game herds and bow-spear-club technique.
Lineage composition.
Autonomous.
Owns territory.
Tribe: Autonomous bands may unite temporarily for religious - social reasons, but lack of food limits durations of supra-band association.

STATUS AND ROLE

Status:

Sex: Male dominance with no variation allowed for ecological differences.
Subsistence: Male economic importance.
Magico-ritual: Shaman very powerful with leader's status.
Inheritance: Generally strict patrilineal inheritance of territory.

Role:

Band: Hunting, ritual, and few other communal affairs.

AUTHORITY

General : Nature: Family:

Band: Leader temporary with slight authority in communal activities.

Location:

Family:
Band: Lineage head (non-formalized as kinship status); shaman.

Legal:

MAN TO SUPERNATURAL

Belief:

Religion is an independent variable determined by diffusion and unique local circumstances. Secondary feature.

Activities:

Supernatural power active agent in many situations.

Agent:

"Shaman" controls a high number of activities. . . feared and respected for supernatural power which often gives him more power than leader.

MAN NON-UTILITARIAN EXPRESSIONS

Independent variable; secondary feature.

MAN TO UNIVERSE

INVENTORY
1976/1977
INVENTAIRE

